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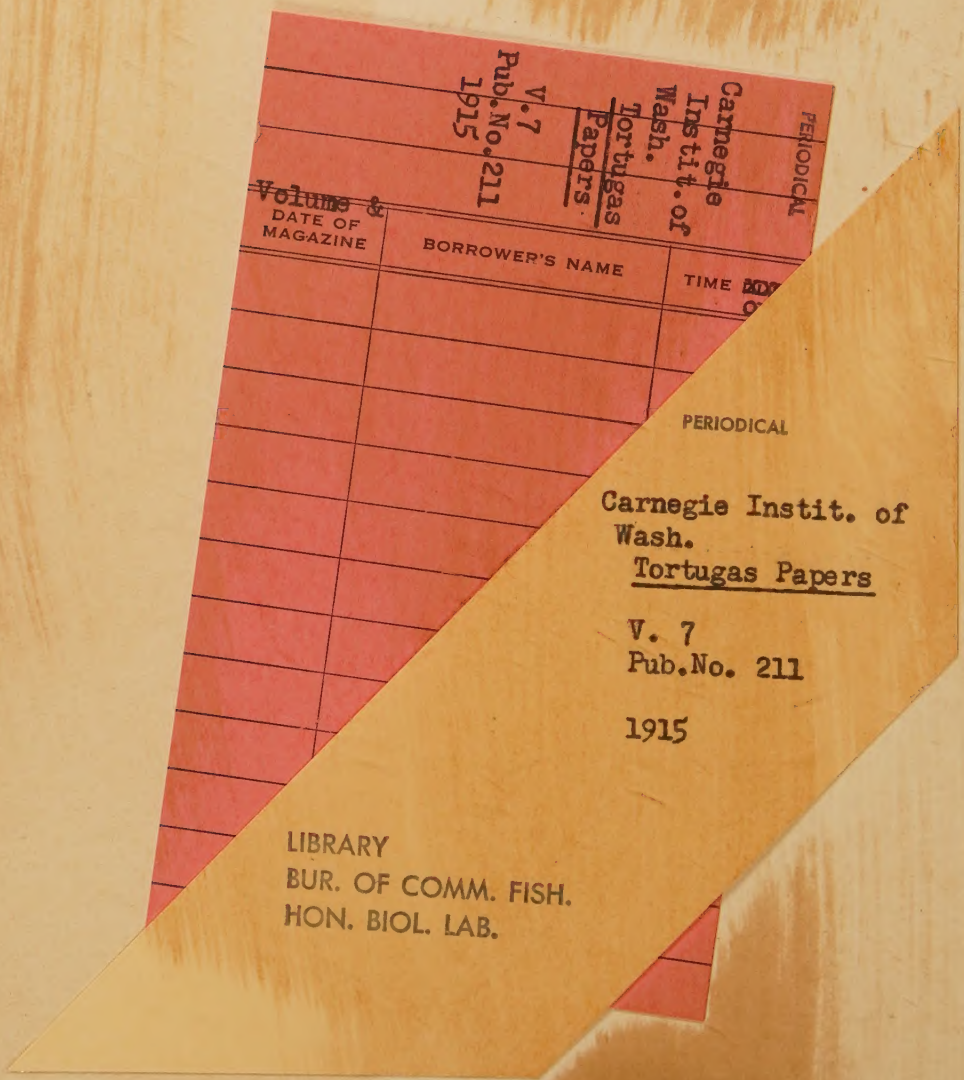

Bird Key viewed from the West, showing certain topographical features of the Key.

PLATE 1

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PAPERS FROM THE DEPARTMENT OF MARINE BIOLOGY
OF THE CARNEGIE INSTITUTION OF WASHINGTON
ALFRED G. MAYER, DIRECTOR

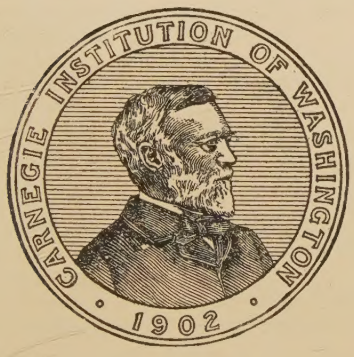
VOLUME VII.

HOMING AND RELATED ACTIVITIES OF BIRDS

BY J. B. WATSON AND K. S. LASHLEY

THE ACQUISITION OF SKILL IN ARCHERY

BY K. S. LASHLEY



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PREFACE.

The present series of studies on the behavior of birds is a direct outgrowth of an investigation made on the noddy and sooty terns nesting on Bird Key, Tortugas, Florida, by the writer under the auspices of the Marine Biological Laboratory of the Carnegie Institution of Washington in 1907 (publication 103). The homing "instinct" is the central topic in all the papers. In the 1907 investigation the fact appeared that terns possess a homing sense, behaving exactly as do homing pigeons when sent away from their nests and young. Such good subjects did the terns appear to be that it was soon decided to make this homing sense the chief object of the Tortugas researches. The topographical situation of Bird Key makes that island especially suitable for carrying out such experiments. In the first place, it is the northern limit of migration of these tropical birds. It becomes possible there to test whether the birds can home in a region never before, in all probability, visited by them. This can be accomplished by sending the birds anywhere north of Bird Key. Secondly, on account of the fact that Bird Key is the last body of land between the coast of Florida and the coast of Texas, the birds can be sent out for hundreds of miles over open water. There is thus afforded an opportunity for testing homing where apparently no "visual landmarks" can exist. These two conditions have never before been realized, and they are conditions which are indispensable in even a preliminary scientific study of homing.

The 1907 investigation already referred to is concerned largely with instincts in terns—those of feeding, nesting, brooding, etc. It was necessary to have some survey made of these instinctive types of behavior before the homing instinct itself could be investigated. A part of the opening paper, pp. 35–45, in the present series gives much additional data on the nesting activities which have a bearing upon the control of experiments on homing. These observations are concerned with the instincts connected with the brooding of the egg, such as the number of shifts at the nest; retention of mate and nest habits; water habits and instincts, etc. It is thought that these observations, when taken in connection with those made in the first study, afford accurate means of testing the homing ability in these two species of terns.

Having made the necessary investigation of the instinctive life of the terns, it became possible to carry out homing experiments. Preliminary experiments in 1910 (p. 47) showed clearly that in order to make a success of the work, some well-trained investigator would have to be sent out with the birds. In the 1910 work the terns were sent out with responsible men attached to the laboratory but who had had no training in feeding and caring for them. On account of this, usually something like half of the birds died *en route*, and the rest were released at the more distant points in very poor condition. In order to meet this difficulty, Dr. K. S. Lashley was asked in 1913 to associate himself with the work. The very successful returns from the second Galveston trip (p. 54) are due largely to his care of the birds on the forward journey.

In addition to his invaluable aid in the problem of distant orientation, Dr. Lashley separately began a study of *proximate orientation* (p. 61), *i. e.*, of the behavior of the terns in locating the nests, mates, young, etc. This paper, in addition to supplementing the work on distant orientation, opens up a wealth

of new problems on bird activities in general, and furnishes methods which may acquire a usefulness much wider than their present connections.

Our experiments and conclusions on homing proper will be found on pages 59 and 60. These results, while not settling the question of the sensory mechanism by means of which the birds return to the nests, *do remove all doubts about the fact that the noddy and sooty terns can return from distances up to 1,000 miles in the absence of all landmarks* (at least in so far as the term landmark is understood at present). The problem of homing has thus become defined, and experimental work of a definite kind is needed for its solution. The paper on vision in birds (p. 87) seeks to gain some insight into the question as to whether the birds have any special visual equipment (such as infra-human sensibility to rays of light, sensitivity to infra-luminous rays, etc.) which might be employed by them in finding their goal. This paper marks only the beginning of the research into the sensory mechanism possibly used by birds in homing. So far we have not been able to locate the sensory factors used in return, but we are far from being without hope that future studies may yield results which will enable us to solve the riddle which has been propounded to scientific men of all ages, but as yet never satisfactorily answered.

The final paper by Dr. Lashley, on the acquisition of skill in archery, has of course no connection with the work on homing. Its inclusion in the present volume is made desirable by the fact that the work was begun at Tortugas under the same arrangements that were made for the other investigations.

Both Dr. Lashley and I wish to express our thanks to Dr. A. G. Mayer, Director of the Marine Biological Laboratory, for his generous assistance in these investigations.

JOHN B. WATSON.

THE JOHNS HOPKINS UNIVERSITY,

February 1915.

AN HISTORICAL AND EXPERIMENTAL STUDY OF HOMING.

By John B. Watson,
Professor of Experimental and Comparative Psychology at the Johns Hopkins University,

AND

K. S. Lashley,
Johnston Scholar in Psychology at the Johns Hopkins University.

A VIEW OF BIRD KEY BEFORE THE HURRICANE OF 1910 DESTROYED THE BAY CEDAR BUSHES.

AN HISTORICAL AND EXPERIMENTAL STUDY OF HOMING.

INTRODUCTION.

In 1907, while the senior author was making a study of the nesting behavior of the noddy and sooty terns at Bird Key, Tortugas, Florida,* the fact was observed that these birds possess a well-developed homing "sense." Only a few experiments were made at that time (see p. 46), but the results were so promising that Dr. Alfred G. Mayer, Director of the Marine Biological Station of the Carnegie Institution of Washington, made special arrangements for the continuation of the work in 1910, 1912, and 1913. Since the homing season is only 5 to 6 weeks in length (being limited to the laying and brooding periods), the present summary of the three years' work is far from being satisfactory. Notwithstanding our failure to work out completely the problems of homing in the terns, we feel that we have made so many improvements in our technique—in the matter of caring for the birds on trips, locating them on return, etc., that it is advisable to report both what we have on homing and on the control of homing experiments. Some care has been taken in the present investigation to supplement the 1907 work on the instinctive life of the terns. This was found to be desirable in view of the fact that in the control of the experiments on homing it becomes essential to have some accurate data on the ability of the birds to swim and to remain on the water at night; the length of time the nesting impulse remains strong; the length of time habits of reacting to one nest and to one mate are retained, etc. The actual experimental material on distant orientation will be given in detail, but the observations upon the instincts and habits of the birds will be presented in a more or less abridged form.

On pages 34 and 35 we give the location of Bird Key, a description of the colony of terns dwelling there, and the complete setting of our experimental work.

Before presenting our own work we shall give in some detail the general historical setting of the problem of homing in vertebrates.

HISTORICAL OUTLINE OF THE PROBLEM OF HOMING.

According to Exner† the problem of homing is a very ancient one. The earliest mention of the use of the homing pigeon is found in the writings of Anacreon (born 550 B. C.). The pigeon post as an institution reached probably its highest degree of development in the reign of the Caliphs—in the year 1200 A. D. For hundreds of years the whole of Persia, Servia, and Egypt was covered with pigeon côtes owned by the government, and attached to each was the official post-office and the official postmaster. That the Crusaders used pigeons to transmit messages to their friends and relatives is also well known.

Probably the use of these birds in times of war, and especially in besieged fortresses, is best known. So important was their use in this respect that

*John B. Watson: "The Behavior of Noddy and Sooty Terns." Carnegie Inst. Wash. Pub. 103, p. 189.

†"Das Räthsel der Brieftauben," Wien, 1892.

until 1850 almost every army post and fort had its côte and was supplied with pigeons from other military fortresses. Indeed, the French extended the use of the homing pigeon to the field by equipping the côtes with wheels (traveling côtes) and training the birds to return to these rolling habitations regardless of their location. They further extended the use of the homing pigeon by establishing côtes on board war vessels. The commercial value of the pigeon post has been very great indeed. Practically all of the boards of trade in the large cities of Europe were once supplied with homing pigeons. Their use in obtaining advance information concerning crops, local insurrections, rumors of war, etc., can hardly be overestimated. Newspapers likewise were supplied with such pigeon posts. A rather lengthy dispatch could be sent, when it is remembered that the message, after it was written, could be micro-photographed in such a way that 50,000 words could be contained on a paper which had a weight of less than 0.5 gram. The dispatch could then be read under the microscope by the receiver.

The decline of the usefulness of the pigeon dates back to the establishment of the telegraph and field telephone lines, and on sea to wireless telegraphy. Even in the present European war, if reports may be trusted, the birds are used to some extent in the scouting work of the balloon and aeroplane corps.

Claparède* has given a very brief description of the theories of homing and of the problems in homing. The theories he classifies as follows:

1. *Magnetic Sense* (Viguier-Caustier?).†
2. *Atmospheric Currents, Winds,* etc. (Toussenel, Ziegler?).
   "Ideas of the Atmosphere" (Thauzies?); *Special Nasal Sense* (Cyon).
3. *Direction of the Sun, of Light* (Romanes, Lubbock, Wasmann).
4. *Special Force* (Fabre); *Attraction of Purely Reflex Origin* (Netter, Bethe); *Tropism* (Loeb).
5. *Registration of Detours* (Darwin, L. Morgan); *Contrepied* (Reynaud, P. Bonnier).
6. *Points de Repère, Topographical Memory* (Wallace, Romanes, Lubbock, Forel, Fabre, Wasmann, Yung, Bouvier, Marchal, Marchand, Buttel-Reepen, Peckham, Rodenbach, Ziegler).
7. *Direct Perception of Goal* (Hachet-Souplet, Duchâtel); *Telepathy* (Duchâtel).
8. *Complex Phenomena Resting upon Intelligence* (Cyon).
9. *Hereditary Topographical Memory* (Kingsley, Parker and Newton).

Claparède classifies the various problems in homing as follows:

Goal
- Already known
  - I. Perceptible.
  - Non-perceptible
    - II. Where landmarks are at hand.
    - III. With no apparent landmarks at hand.
- Unknown
  - IV. Perceptible.
  - Non-perceptible
    - V. Where intermediary stimulations are at hand.
    - VI. With no intermediate excitations.

*Archives de Psychologie, II, 1906, p. 133.

†Where an author is hard to classify an interrogation point is placed after his name. In many cases a given author makes use of more than one theory to account for the different phenomena of homing. Many authors whose names appear in the above lists are not again mentioned in this report. Reference to their work may be found in Claparède's bibliography. This bibliography, which is fairly complete up to 1906, is not duplicated here. Many references supplementary to our own are to be found in it.

It will be seen from this table that Claparède's treatment is far too broad to be of particular service to us in discussing the narrow division of the field of orientation which especially interests us in the present paper. Claparède includes in his discussion both vertebrates and invertebrates, both proximate orientation and distant orientation, and includes under the latter term all of the phenomena of migration. In our historical summary we shall deal with homing in *vertebrates* where the *goal is "already known."* At the outset it seems best to divide the subject of homing into two parts: (*a*) proximate orientation and (*b*) distant orientation.

PROXIMATE ORIENTATION.

The term proximate orientation ought to be understood as referring to the process of finding the goal (since we are in need of some general term covering nest, burrow, côte, etc., it seems well to let the word goal refer to any of these) when the goal itself directly and immediately stimulates the receptors of the animal. In other words, in proximate orientation vision, olfaction, audition, or some other distance receptor is being immediately stimulated by the goal. We might illustrate the process of proximate orientation by citing the case of the young bird which does not go out of sight of the goal to feed, and of young mammals which stay within call of the parent. It might be equally well illustrated in cases of animals with keen smell, which apparently can journey quite a distance away from the goal and still return to it by reason of the olfactory stimuli which emanate from it.

An interesting case of proximate orientation occurs thousands of times in the course of a single evening on Bird Key. As has been mentioned, the sooty tern often flies around the island at night. When it is remembered that the island is quite small, that there are probably 30,000 sooties there, and that they nest oftentimes not more than 10 inches apart, the problem of their return at night after these short journeys becomes a puzzling one. As a matter of fact, the bird hovers over the nesting area giving out his call; he is answered by his mate and young and is thus guided to the nest, often on dark nights. In this paper, then, proximate orientation will refer to those cases where orientation is effected through the action of a stimulus which emanates from the nest (sound, light, olfactory particles, etc.).*

DISTANT ORIENTATION.

Distant orientation involves always a return to the goal from a distance too great for the goal itself to function as a direct stimulus to any known receptor of the animal. It is obvious that the return in such cases can be effected only in one of two ways: (1) the animal may be guided back by a series of landmarks to which it has already established reactions (*points de repère*, "visual landmarks," "familiar landmarks," etc.). Such landmarks may appeal to any sense-organ possessed by the animal. If it is keen-sighted, we may suppose that the landmarks would be visual; if highly sensitive to olfactory stimuli, olfactory, etc. Those who support the landmark theory of homing usually suppose that there is a series or chain of such stimuli so placed that before one stimulus ceases to act another presents itself. It is obvious that

*On certain phases of proximate orientation, see Lashley, page 61 of this volume.

the animal reacts to each of these serial landmarks as it would react to the goal itself. An obvious logical implication is that the animal—granted that it moves about at random until the first of these landmarks is reached—must be able to follow these clues in the direction towards the goal and not in the reverse way (in the case of a dog, *e. g.*, there must be no back-tracking). Were this not the case, the animal, approaching the chain of landmarks at right angles, by turning, *e. g.*, to the right, would meet a series which, if followed up, would take it home; if to the left, a series which would lead it still farther afield. We can conceive, under this hypothesis, of an animal being effectually lost in a wilderness of landmarks. In other words, if orientation is obtained by reference to landmarks, the trail must be polarized. It is also obvious that this hypothesis calls, first, for the ability on the part of the bird to form habits of very great complexity; second, that it must form them with extreme rapidity to account for the facts of homing (even in cases where landmarks are possible). For convenience we shall group all such theories under the heading of "habit theories of return." Such theories are advanced by Hachet-Souplet, Hodge, and others.

It is clear that if it can be proven that birds can return only over stretches which afford landmarks, then there is no real problem of distant orientation at all. The problem simplifies itself to the determination of what sense-organs the animal employs in its normal habits of reacting to objects, the intensity of the stimuli which call out such habits, and the rapidity with which such habits are laid down, etc. In essence, the problem is as simple as determining whether a young animal goes to its nest by reason of the presence of an auditory stimulus (cry of the parent) or through vision (sight of the nest); or gets out of the way of a hawk through the sight of the hawk or its shadow, or through the auditory stimulus set up by the crying of its neighbors.

On the other hand, the animal conceivably may be guided back reflexly through the action of some unknown intra-organic or extra-organic stimulus, acting upon some assumed but unknown sense-organ. We will call this hypothesis (2). We shall develop this conception in connection with the "contrepied theory" of Reynaud, the attitude theory of Bonnier, and the magnetic sense of Viguier and Thauzies, etc. In such reflex returns no special locality or position habits have to be formed. No landmarks are needed. All such theories we shall group under the heading of "reflex theories of return."

In the historical sketch which follows we have not tried to do justice to all theories nor to all experimental data. We have tried to present the more important theories and where possible some of the experimental details upon which they are grounded. We have spent a disproportionate amount of time upon the historical aspect of homing in vertebrates because no one hitherto has attempted to treat the subject in monographic form.

HABIT THEORIES.

THEORIES AND EXPERIMENTS ON THE FUNCTION OF VISION IN HOMING.

Hachet-Souplet* advances some positive proof that vision furnishes the data for distant orientation. He has made a number of experiments with traveling côtes. These traveling côtes are used quite commonly in France and in Belgium. We give here his account of some recent experiments:

> After having put in a basket and left at a point (A) a part of the inhabitants of the traveling côte, the exterior aspect of which was known to the birds, we sent the carriage forward to a distance of about 5 km. from the point A. The pigeons, released as soon as the carriage arrived at B, found it very rapidly. We have repeatedly tested this, increasing the distance from A to B up to 10 km. We have found it necessary to place the carriage, which is supplied with a large drapery, in an open territory. The birds have always found it. If this distance of 10 km. is passed we begin to undergo losses of the birds, and we have never obtained returns beyond the distance of 12 km. It is necessary to consider here that the birds know nothing of the *entourage* of their côte. If, proceeding otherwise, we make the carriage stationary on its arrival at the new place and allow the birds to reconnoiter the immediate environs before placing them in the basket (in accomplishing this we attach the birds to the carriage by cords which allow them to fly up to a height of 35 m.; in order to avoid accidents one attaches only two birds at a time) we have been able to obtain on the first attempt, without preliminary training, a return of 100 km. This was accomplished by 8 pigeons. At the time this test was made we had sent to the same point of release (100 km.) 10 pigeons which had been brought in the caravan without being allowed to leave it (that is, they were not allowed preliminary observation). All 10 of the birds were lost. This experiment was repeated with the same result several times.

Hachet-Souplet supposes that the view from the carriage supplied the birds with a set of "visual memories" which they could utilize—*i. e.*, that orientation is effected through sight. With regard to the limits within which a bird can return through direct sight he states that the limit is much further than it is at present believed to be. He considers what he calls the "law of the diminution of the necessary intensity of the excitant which determines the reaction." (He has in mind here, of course, decrease in the intensity, etc., of the visual stimulus through distance.) Naturally one would expect that a visual excitation (*i. e.*, one accurate enough to assist in orientation) would cease at the point where a clear image ceased to be formed on the retina. But the disappearance of a clear image on the retina does not mean a lack of visual excitation, according to Hachet-Souplet; for, as the bird gets further and further away, the clear image gives place to an "*imprecise*" visual impression, corresponding less to the perception of form and determined colors than to a sense of "*déjà vu familier.*" The impression becomes "mixed" in the sense that it bears elements of many different objects. He likens the view that the bird gets of a distant goal to our view of a distant forest. When one looks at a distant forest one does not see the leaves, and not even the trees separately. This author thinks that the "mixed impression" is serviceable at a much greater distance than is ordinarily supposed, and he states that refraction adds greatly to the distance at which it may function. He maintains that all of the calculations showing the heights to which birds must fly to see the côte have been exaggerated because they have not been properly corrected for refraction. Given the curve for refraction, it will be possible for the birds to obtain these

*Hachet-Souplet: VI. Cong. Int. de Psy., 1909, p. 663.

"mixed impressions" at all the distances from which they ordinarily home. It is well to note (1) that it is not during the forward journey that these "memories" are graven in the memory of the bird (they come from previous flights), and (2) that these memories which are able to be considered as effective are not memories of precise form, but of special impressions of *déjà vu*.

Hachet-Souplet gets around many of the facts by denying them. He says:

> One fact which appears out of harmony with the theory of orientation by view are the returns from 800 to 1,000 km. which pigeons have effected without training in a direction entirely unknown to them, but these facts are controverted for the most part and are always exaggerated.

Where the facts are incontestable he would explain them on the basis of chance. Most of the flights which are considered in our own paper are far beyond this limit set by Hachet-Souplet.*

Schneider's† conclusions are numerous, the one of chief interest being as follows:

> The assumption that the carrier pigeons possess an inborn sense of direction is an error; for if this assumption were true, then the young pigeons ought to find their way equally well. The investigations have shown, especially those at Könitz, that young pigeons, even at relatively small distances from their home, have the greatest difficulty in finding their way back when the vicinity is at all strange to them, and their home can not be directly seen.

He then concludes that the young birds utilize, in their early flights, the familiar groups of houses, mountains, etc., and that the distances to which a bird may be taken safely may be increased commensurately with the increase in the development of his "topographical memory." This author believes that the pigeon can develop not only "Errinerungsbilder" but even "Gedächtnisse." Schneider had had wide practical experience before reaching these conclusions.

Like the previously considered observers, Hodge‡ finds no necessity for assuming a sense of direction in the homing pigeon. His experiments were made largely upon young pigeons. When they were first released at the côte they circled about the côte, then flew from one distant point to another, and thereby (according to this author) established a system of *visual landmarks*. Young birds taken in an open cage half a mile away from the côte returned with few circling flights; but young birds removed in closed cages circle, make errors, and even fly for long distances in a wholly wrong direction. Training produces almost immediate results in good birds. Only after training do they take a straight line and behave as though they had a sense of direction.

When placed in an unknown neighborhood the bird begins upon a "line of search." "The logical curve of search is a peculiar spiral, the involute of a circle, the characteristic of which is that the convolutions are always the same distance apart. These distances will be, of course, twice the distance at which the object is visible."

---

*According to Sébillot, quoted by Hachet-Souplet, when homing pigeons are released at the sea the altitude at which they fly increases proportionately to their distance from the land. At 146 km. the pigeons scarcely pass over the normal altitude of 150 to 300 meters; at 200 km., they had visibly mounted much higher; at 300 km., the altitude was found to be at least 600 meters. In the final test we lose the birds from view in the heights and not at the horizon.

†Schneider, G. H. Zeitsch. f. Psychol. u. Phy. d. Sinnnes., 1905, XL, pp. 252–279.

‡Popular Science Monthly, 44, p. 758.

In accordance with this theory Hodge finds as follows:

Of a large number of birds sent away by rail for longer flights, not one made the return trip in less time than would have enabled him to fly on the involute of a circle from the place of liberation. This refers to "first flights." The best time made was 26 miles in 5 hours and 9 minutes. Three other birds liberated at the same time failed to return. Letting the distance to which a prominent landmark is visible be 3 miles, a fair estimate for pigeons with some training and over broken country, the least length of an involute of a circle which would bring a bird from a distance of 26 miles to within sight of the loft is 219 miles. In 5 hours it is not likely that the pigeon flew less than 250 miles.

Duchâtel* advances the highly speculative view that the retina of the bird is sensitive to infra-luminous rays, especially infra-red, the waves of which are supposed by him to follow the surface of the earth, and consequently are not interrupted by its sphericity. In answer to this speculation we are now in the position to state that the retina of the homing pigeon is no more sensitive to these rays than is the retina of the human being. It was to meet Duchâtel's supposition that we undertook the work on spectrum sensibility in birds, the result of which is shown on pages 87 to 104 inclusive.

EXPERIMENTS SHOWING IMPORTANCE OF VISUAL IMPULSES IN HOMING.

It is generally agreed that pigeons home with difficulty in cloudy weather. Rodenbach† released 6 good pigeons at 10 a. m., 30 km. from their côte. The sun was obscured by clouds. None of the birds returned the same day. Two returned the second day after the clouds had partly disappeared; two on the third; and two failed to return. Furthermore, Hachet-Souplet‡ shows that they do not home at all at night. He released pigeons at different hours of the day as follows:

No. 1 at 8 a. m., returned at $8^h\ 25^m$.
No. 2 at 10 a. m., returned at $10^h\ 20^m$.
No. 3 at 12 m., returned at $12^h\ 20^m$.
No. 4 at 4 p. m., returned at $4^h\ 30^m$.
No. 5 at 8 p. m., returned at $4^h\ 15^m$ the following morning.
No. 6 at midnight, returned at $5^h\ 5^m$ the following morning.

From this Hachet-Souplet draws the conclusion that the birds are guided back to their nests by an "*attraction visuelle.*" He neglects to take into account the records we have upon birds which migrate at night.

Rodenbach also made a test on a blind pigeon. After recovery the pigeon was placed before the entrance to its côte. Guided by the voices of its companions it was able to enter the côte and to beat its way to the right or left. On the following day it executed this feat more easily. On the third day it was taken some distance from its côte. Again, guided by hearing, it joined its companions on the roof. With great difficulty and by making desperate efforts it was able at the end of 2 days to reënter the côte. Some days later the bird was released in an open field at a distance from its côte which a sound bird could cover in 10 minutes. The blind bird flew vertically upward without circling, then to the south in a direction opposite to its côte. The bird never returned. These results agree with those obtained by Hachet-Souplet upon good fliers which had become blind. These birds, when released within 4 leagues of their côte, flew away and did not return.

*Ann. de psych. zool., I, 1901, p. 22; II, 1902, p. 48.
†Rodenbach. Zeitsch. f. Brieftaubenkunde, 1895, p. 134.
‡Hachet-Souplet. Annals de psych.-zool., II, pp. 33–60.

EVIDENCE AGAINST HACHET-SOUPLET'S THEORY BASED UPON MATHEMATICAL CONSIDERATIONS.

The evidence we offer below militates against any theory of return over long distances at sea based upon vision. It immediately discredits Hachet-Souplet's theory of "direct perception of the goal" (as applied to the terns).

The distance at which an object is visible at sea is expressed approximately by the formula $d = 1.317\sqrt{x}$ in feet, where d is the distance of the object in statute miles and x the height of the object above the earth's surface in feet. The altitude obtained by this formula is, for shorter distances, approximately that obtained by calculating the tangent to the arc of the distance and correcting for the mean refraction at the horizon (36′ 29″), but for great distances the value obtained is somewhat too great. However, for our problem this error is negligible.

TABLE 1.

Distance.	Height to which bird must fly to meet light-waves reflected from land lying just above sea-level.	Height to which bird must fly to meet light-waves reflected from top of lighthouse.	Least object visible, 30″ acuity.	Least object visible, 4′ acuity.
statute miles.	*feet.*	*feet.*	*feet.*	*feet.*
18	186	3.4	137.0	1,100
65	2,430	1,407	496.0	4,012
	miles.	*miles.*		*miles.*
105	1.2	0.9	792.0	1.2
191	4.0	3.3	1,415.0	2.2
269	8.0	7.0	2,059.0	3.1
300	9.8	8.8	2,270.0	3.4
400	17.4	16.1	3,062.0	4.6
500	27.3	25.6	3,801.0	5.8
582	36.9	35.0	4,435.0	6.8
			miles.	
700	53.5	51.1	1.0	8.1
800	69.9	67.2	1.2	9.3
900	88.4	85.4	1.3	10.4
1,000	109.2	105.8	1.5	11.6
1,100	132.1	128.4	1.6	12.8

The second column of table 1 gives the height at which a bird must fly in order that its eye may meet with rays reflected from an object at sea-level at the distance indicated in the first column, provided that the bird is stimulated only by light waves ranging from 400 $\mu\mu$ to 700 $\mu\mu$ in length. The lighthouse upon Loggerhead Key is 151 feet in height. The third column of the table gives the height at which a bird must fly in order to see the upper 10 feet of the light tower at the distances given. Even upon the assumption that the birds have absolute visual acuity, it is evident that they can not fly high enough to be guided by light reflected from the goal for more than a small fraction of the distance through which they find their way back. But the limitations of acuity are probably even greater than those imposed by the curvature of the earth. For man two points of light are not distinguishable unless they are separated by at least 30″ of arc. Hence for a coast-line to be

recognizable to man it must present inequalities of elevation which amounts to at least 30″ and these must be relatively abrupt.

The fourth column of table 1 gives the least observable inequality in the coast-line for the distances in column 1 for man.

The only data upon acuity in birds has been presented by Johnson* on the chick and places its acuity at about 4′. The fifth column in the table gives the smallest inequality in the coast line which can stimulate such an eye at the distances indicated in the first column. The most irregular mountains in the region of the Gulf of Mexico, where our observations were made, the Sierra Madre, would have, at a distance of 300 miles, the same stimulating value for the eye of this bird as a fine straight line seen a few feet away, the irregularities being lost in the imperfections of its dioptric mechanism. If the visual acuity of the homing bird is no greater than that of the chick, or even of man, the possibility of its reaction even to an "imprecise" visual impression seems precluded for distances of more than 100 miles.

A THEORY BASED UPON CUTANEOUS SENSIBILITY.

Cyon,† although he has contributed largely to our knowledge of the structure and function of the labyrinth, does not wish to be confused with those writers who attribute to them any special function in distant orientation. Naturally they are brought into play in the flight of the bird, but only as a reflex mechanism involved in the maintenance of equilibration.

In 1897 Cyon first announced his own theory. The theory advanced posited a *Spürsinn*, which has its location in the nose. This sense may be wholly independent and separate from the ordinary olfactory sense. In support of his theory Cyon refers to the well-known fact that the hunting-dog can at great distances find a person to whom he is attached. Why should not the homing sense be bound up in other animals with this sense? The fact that the sense of smell is so little developed in birds can mean nothing but that they are not sensitive to odors which affect the human being. This fact does not militate against the view that the organ of homing may have its seat in the same organ, without being identical with the olfactory organs. Many people from their youth are lacking in the sense of smell; they show a remarkable inability to orient themselves, not only in cities with which they are most familiar, but also in large buildings, especially if the latter are laid out symmetrically. They require time to orient themselves in a new building. Cyon suggests also that the work of Bethe on ants and bees seems to speak for a division in the olfactory sense.

Cyon's preliminary experiments showed that the birds were insensitive to ordinary smell substances. The birds showed no reactions when oil of cajuput and asafœtida were offered. On the other hand, they were found to be so very sensitive to ammonia that it may be used to test the anæsthetizing effect of cocaine. Fanciers sometimes hold that pigeons react to the nest by reason of smell. To see if this was well founded, Cyon blinded a brooding pigeon by closing the eyes with cotton soaked in collodion. The experiments were carried out in a house the third story of which contained a côte. The

*Journ. Animal Behavior, 1914, p. 340.

†Cyon, E. von: Ohrlabyrinth, Raumsinn und Orientirung, Pflüger's Archiv, LXXIX.

bird remained immovable. She only flew when tossed up in the air. In spite of the open door, she made no effort to return to her nest. Even in the vestibule she made no attempt. She did not return to the nest if placed just in front of it, but remained on the spot. This test shows only that the ordinary olfactory sense is not acute in the pigeon.

On the basis of an experiment to be described, he advanced the view that *the direction and temperature of the wind act upon the olfactory mucous membrane.* As we understand him, his theory is based, on its sensory side, upon the functioning of the tactile nerves (largely V^{th}) in the nasal cavity.

Cyon's experimental data in support of this theory are exceedingly limited. He selected 3 young homing pigeons of equal age and ability, which had been trained to return from distances of 4 to 500 km. (direction not stated). Pigeon A was used as a norm. Pigeon B had the anterior nares closed with a cocaine-saturated plug, the whole coated with collodion. In the same manner the auditory meatus of pigeon C was closed. The 3 birds were carried at 7^h 30^m a. m. from Spa in a railway carriage to the station at Huy, which by rail was 60 to 70 km., and in an air-line, 50 to 55 km. from Spa. Huy was chosen because it lay in a valley and was separated from Spa by a mountain. The weather was clear and fine. Pigeon C did not stand the trip very well and ate apathetically the day before release. They were released in the following order: A, at 10^h 13^m a. m.; B, at 10^h 19^m a. m.; C, at 10^h 26^m a. m. There was some difference in the orienting flight. The norm, A, arose perpendicularly and began to make the characteristic circular flights. It then took suddenly the direction of northwest, following the line of the railway. B ascended fully as rapidly, but not perpendicularly, veering towards the east. Finally it took the direction south. C was released 7 minutes later and arose with slower speed; its circles were smaller; oftentimes, in place of going higher, it descended. It took the direction east. Pigeon C (auditory meatus closed) was the first to reach home, at 11^h 35^m. It required, therefore, 1 hour and 9 minutes for the trip. The normal pigeon appeared at 12, requiring 1 hour and 47 minutes. It probably chose the railway back, going through Lüttich and Peppinster, while C probably chose the air-line. Pigeon B (nasal passages closed) appeared 4 days later, September 6, between 4 and 6 p. m. This bird required 78 to 80 hours for the trip. It was found that the nasal passage was completely clear, both of the cotton and collodion.

Cyon does not believe that this belated return was accidental, nor that the pigeon was held in durance, because a large price was offered for a report of it. He feels that the bird wandered more or less aimlessly until it could free its nose of the plugs, after which it sought its way home. Cyon's position may be summarized as follows: (1) Orientation, in the last analysis, depends not upon purely instinctive and reflex processes, but upon intelligent adjustment (habits based largely on cutaneous sense). (2) Orientation is attained with the help of two senses: " . . . des Gesichtsinnes und eines speciellen Spürsinnes, der in der Schleimhaut der Nase (und vielleicht der Stirnhöle) seinen Sitz hat. Letzteren Sinn kann vom Geruchsinn unabhängig sein. Er wird vorzugsweise durch die Qualitäten der Winde (Richtung, Intensität, Temperatur, . . .) in Thätigkeit versetzt." (3) The vestibular portion of the ear functions only as an equilibratory mechanism.

EVIDENCE AGAINST CYON'S "SPÜRSINN THEORIE."

On June 16, 1910, a test was made at Tortugas of Cyon's hypothesis which has just been considered. Three noddy terns were captured on the night of June 15. On the morning of June 16 the anterior nares were first sponged out with alcohol to remove all secretion, and then filled with warm wax, packed in. The whole was then coated with asphaltum. The feet of the birds were tied and they were put back in the cages until the asphaltum had had a chance to dry. The feet were left tied also during the transit of the birds, since this prevented them from scratching at the wax and gave them a chance to become habituated to it. Two of the birds, in charge of Mr. Rufus Mills, were sent to Key West and the third (the control) was sent to Loggerhead, 3 miles away. The Key West birds were released at 2 p. m. The nests were observed that night, but apparently they did not return. At daybreak the next day the two birds were both found upon their nests. Since the nest mate was just leaving one of these birds, I am inclined to think that at least one got back on the same afternoon in which it was liberated. The control bird was released at Loggerhead at 7 p. m., in plain sight of Bird Key, with head directed towards Bird Key. Drs. Mayer and Treadwell were both present. This bird flew to the water and bathed, then flew slightly eastward in a straight line to Bird Key; it was on the nest when the experimenter reached there, 30 minutes later. It was found that the plugs in the anterior nares were present in all three birds. The asphaltum was not even scratched and had hardened completely. The birds had resumed their nesting activities. This test (so far as the terns are concerned) seems to be absolutely conclusive against Cyon's hypothesis.

REFLEX THEORIES.

REFLEX THEORIES BASED ON THE FUNCTIONING OF SEMICIRCULAR CANALS.

Reynaud,* although having at his command the best facilities for making scientific observations, has contributed little on the factual side to the study of homing. For some years he had charge of the movable côtes of the French army, and later of the carrier-pigeon post of "La Compagnie Transatlantique." His theoretical views have been crystallized into a "loi du contrepied." This law merely states that the pigeon returns by the route over which it was carried, retracing, on return, all of the windings of the forward trip. He tries to get some factual support for this law by keeping a record of the finding of lost birds. The birds at times disappear and are usually found, according to him, at some point on the route over which they have been carried. His records, however, are extremely sporadic and are of no scientific value. He assumes, furthermore, that there is proof for his theory in the great migration routes of birds and in those of fishes. He maintains that both the forward and the return migration routes are the same in every case, and that certain species of both birds and fishes have practically been exterminated by sportsmen because of their adherence to these routes. That such is not often the case has been quite clearly shown by Cooke, who proves very plainly that in many cases Northern and Southern migrations do not take place along the same route. The most notable example of this is to be found in the plover (see p. 29).

Like a good many others, Reynaud places the mechanism of return in the semicircular canals. The stimuli to it, although not clearly defined by him, are practically the same as those of Bonnier. The twists and turns which the bird receives as it passes over the forward route are stored up in some way so that they become effective as stimulators when the bird returns (see Bonnier's attitude theory).

A crucial test of Reynaud's theory could easily have been devised by him by taking the bird along an extremely circuitous route and then releasing him, *e. g.*, not more than 300 miles from his home. If the bird returned at the end of 5 to 6 hours, we may be sure that it returned by the direct route. If, on the other hand, several days were consumed, we might have some grounds for supposing that the longer route was chosen. He maintains that he has made such tests, but no satisfactory publication of them has ever been made. (See reference to other articles by Reynaud, in Claparède's bibliography.)

Bonnier† advances a theory (no experiments given) which may be classed, despite its appeal to memory, as a reflex theory. It was put forth after the contrepied theory of Reynaud and does not differ from it except in attempting to account for direct returns (that is, for known cases where the bird does not retrace the various stages of the forward journey, but on the return takes a direct route). According to Bonnier there are two general methods of orientation. One is able to orient oneself on the moment of arrival or at the instant of departure. In order to orient in advance, or at the moment of release, landmarks (*repères*) of one kind or another are necessary. But in many cases the conditions are made such that landmarks are not at hand. Hence, Bonnier believes that orientation is always obtained by reference to the *point*

*Bird Lore, 2, pp. 101–108; *ibid.*, pp. 141–147.
†Bonnier, P.: L'Orientation, Scientia, No. 9, 1900.

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of departure. The point of departure is a landmark (*repère*) forcibly known to us, since we go directly from it. By remembering all of our displacements since our departure we remain, after a fashion, in continual contact with this point. Whenever we are distant from it we strive, without keeping detailed accounts of these displacements (even unconsciously), to maintain at every moment the notion of its direction with reference to the course of our displacements. The consciousness of the point of departure is a fundamental base and suffices for all our operations. The idea of its orientation, that is to say, of its displacement with reference to us, increases in clearness just in proportion as we feel that we are displaced with reference to it. This idea is furnished by the operation of the sense of total "aptitude" (*aptitudes totales*), the ampullar sense of our 3 labyrinthian canals ("*. . . le sens ampullaire de nos trois canaux labyrinthiques*"). The memory which supplies this notion fixes it and preserves it just in the proportion that memory itself develops. This memory is but an aptitude, not more astonishing than many others, and it is natural that this aptitude should have acquired, by hereditary accumulations, an extraordinary power in the species in which the exercise of this instinct is a condition of survival and an important factor in evolution.

We have given above as faithful a presentation as we can gather of Bonnier's position. We have attempted where possible to give a free translation of his writing. So far as he advances the hypothesis of orientation by appeal to displacement, we presume Reynaud would agree with him. Bonnier then advances the idea that individual birds differ in their capacity to orient, as follows:

> The homing pigeon, transported in a basket, holds fast to the memory of the direction of the point of departure (in spite of the various successive passive displacements). Arrived at the point of release, he has not for a moment lost the precise notion of the component displacements or of the total displacement. This notion, condensed by memory, is like the thread of Ariane which he attached to his point of departure. If he is not quite sure of himself he retraces stage by stage the road which bore him to the distant point—but if he is expert in distant orientation, he holds directly to the thread in place of following its twists and turns. He relies upon direct orientation, on the notion of the point of departure, an idea which he guards in proportion to his displacement, relying among all these detours only upon this constant idea of the sense of return, or orientation from the point of departure [*i.e.*, as we understand Bonnier, the bird resorts to a kind of triangulation process]. It is to be presumed that expert birds in distant orientation have a more highly organized hereditary memory.

Bonnier's aptitude or displacement theory, if such we may call it, must be looked upon as a suggestion, and one which is extremely vague at that.

REFLEX THEORIES ASSUMING MAGNETIC SENSIBILITY.

Viguier,* basing his views upon the "absurdity" of supposing that a bird can fly high enough to perceive its côte when at long distances at sea, and that migrating fish can run their course through vision or smell, maintains that the physical force which enables animals to direct their course must be everywhere present, "in the heights of the atmosphere as well as in the depths of the sea." This force is terrestrial magnetism. To the objection (already raised by Darwin) that a compass without a chart avails but little, Viguier replies:

> Is this objection insurmountable? I think not. The compass gives to our mariners a simple direction. They need to know the angle that this direction makes with their route

*Viguier, C., Revue Philosophique, 14, p. 11.

in order to direct themselves. . It is the chart which serves this purpose; and if one conceives of animals endowed with a sense which permits them to recognize only the direction of magnetic north, these animals would have a constant notion of north, but would not be capable of returning to their point of departure unless they had maintained an exact memory of the length of the lines traversed in this or in that direction. The mariner, navigating by dead reckoning, has to write down the length as well as the direction of all his rhumb lines. Such a view demands at least a constant perception and a prodigious memory and does not correspond at all to the simplicity of the notion of which I spoke above. But terrestrial magnetism furnishes other indications. If we suppose that an animal is capable of perceiving influences which actuate the needle of the compass of inclination, and that the position of this animal remains always the same with respect to the vertical of the place, he will be able to recognize the differences in intensity which cause the variations in the angle which we measure with the aid of this compass, an angle which varies, as we know, from 0 degrees at the magnetic pole to 90 degrees at the magnetic equator. He would be able, then, to recognize the direction where this angle varies most quickly (magnetic meridian or isogonic line), and where it remains constant (magnetic parallel or isoclinic line). Finally, there are other data which we may gather from the use of our instruments, and which one can suppose perceptible through an appropriate organ. We have in mind variations in magnetic intensity. If one disturbs the position of equilibrium of the needle of the compass of declination, the needle comes back, by a series of oscillations, which show a variable force according to the place. By joining all the points where this force is equal we can trace on our maps a series of lines (isodynamic) which can be confused neither with isogonic nor with isoclinic lines (although they show a development somewhat like that of the latter). One can then admit that a line can be determined for an animal, endowed with the magnetic sense as complete as we are assuming it to be, by the value of the magnetic actions in inclination and declination, just as fairly as it would be determined for us by the intersection of given magnetic lines. These magnetic conditions once known, the animal will be capable always of going directly to this point. Carry it to considerable distances, cause it to describe complicated circles, anæsthetize it even during transport, it will be able always to come back to its point of departure. This sense is supposed to give the animal general direction. It will suffice to bring it into a neighborhood where the five senses can be employed.

He supposes that the magnetic currents act in such a way as to release reflexes. The tissue affected is specified as follows:

Assume, then, that terrestrial magnetism induces true currents in the endolymph of the canals, the intensity of which varies with the position of these canals with respect to the directions of the needles of inclination and declination, and with the intensity of the magnetic phenomena.

Thauziès,* likewise, as is well known, believes that the magnetic theory is the only one which will account for the various facts of homing. In his description of this theory he makes the following statement:

The pigeon possesses a *sensibilité magnétique* which permits it to receive impressions quite special and even somewhat mysterious—terrestrial magnetic currents, lines of force, etc.—the influence of which combined with the work of the other senses puts the animal on the true route and enables it to maintain it.

Thauziès favors this theory because it is "logical" in appearance and because it brings into play the "mental activity" of the bird. Thauziès has had an immense amount of practical experience.† For 23 years he has made

*Thauziès, A.: L'Orientation lointaine, VI. *Cong. Int. de Psy.*, 1909, p. 263.

†He states that the distance from which a pigeon can return is rarely greater than 500 km. As a matter of fact, most of his flights lie well within this distance. He tells us further that, so far as he knows, this faculty of orientation is not lost through age. At the age of 20 years a pigeon is no longer fit for service. The quickness of flight attains to 110 to 120 km. to the hour, but it varies according to the direction which the pigeon has to follow. These statements were made in reply to questions asked at the time the paper from which we quote the above was read.

observations at his côte in Perigueux. He gives little material which is serviceable in solving the problem of homing. One statement he makes which is useful is that the birds rarely fly higher than 300 meters; the average height is 150 meters. If a mountain range causes them to fly high in passing they immediately fly down again and stay near the surface of the earth. The only really positive evidence which Thauziès advances in support of the theory comes from the parallel records which he has kept of disturbances in flight and of disturbances in terrestrial magnetism. We summarize the facts from two cases:

On August 18, 1907, the Société Colombophile, of which I am a member (L'Hirondelle de Perigueux), released at Orleans (320 km.) at 6^h 30^m a. m., on a bright calm day with slow south wind, 99 young pigeons which, up to that time, had given perfect satisfaction." ". . . this journey, which we would have thought extremely slow if completed before mid-day, was not completed by any bird before 2^h 43^m. Only 11 subjects came back before night, the rest the following norning. It was so abnormal that we made inquiries of others. We learned that most of the societies in these regions who had pigeons *en route* had been as surprised as we ourselves were by the tardy home-coming of their birds. The year preceding, on July 22, we had been victims of a yet more singular misadventure. Our young pigeons released at Angoulême (65 km.) at 10 o'clock under an admirable sky, limpid and without wind, required more than 3 hours to make the journey, which should demand scarcely an hour. Many birds were even later than this and many were lost altogether. On taking up the matter with the Astronomical Department we found that on July 22, 1906, and on August 18, 1907, there were severe electrical storms.

Thauziès's observations are incomplete because of the fact that there may have been many just as violent electrical storms which produced no retardation in the flight of the birds. Until the test has been worked both ways, one can not draw any conclusions, and even if an electrical disturbance did produce a retardation of flight, this would not necessarily imply that the birds were using terrestrial magnetism as a means of orientation. There are possibly other phenomena connected with electrical disturbances which may have produced the effect. The correlation, if it could be made, would be exceedingly interesting.*

EXPERIMENTAL EVIDENCE AGAINST THE REFLEX THEORIES.

Exner† has made the most careful experiments to be found in the literature on the conditions which affect flight. If his results could be confirmed under slightly better conditions they would certainly discredit both Reynaud's theory and certain of the theories based on magnetic sensibility. He tried the experiment of stimulating the birds with an electrical current during the forward journey; anæsthetizing the bird, rotating it, etc. His experiments were carried out near Vienna. Several experiments of the following kind were made: birds were carried in a box arranged for the giving of electrical stimulation; electrodes were placed upon the head in such a way that a current could be made to pass through the semicircular canals, etc.

Quer durch den Kopf geleitet, bewirkt ein galvanischer Strom bekanntlich bei Mensch und Thier Schwindelgefühle, das heisst die Empfindung einer Drehung oder Neigung, deren

*The U. S. Weather Bureau at Washington has offered to coöperate with us in attempting such correlations. It may be possible to warn an observer in advance of the possible approach of an electrical storm (approach of sun-spots, etc.). It would seem feasible, with such a warning, to ship the birds several hundred miles and then release them and note the returns.

†Exner, Sigmund: Negative Versuchsergebnisse über das Orientirungsvermögen der Brieftauben, Stizungsberichte der Kaiserlichen Akademie der Wissenschaften (Wien), p. 318.

Sinn von der Strommesrichtung abhängig ist. Es ist das der sogenannte galvanische Schwindel, auf den hier einzugehen nicht der Ort ist. Bei der Taube verräth er sich durch Neigungen des Kopfes, Augenbewegungen u. s. w. Ich suchte also während der Hinfahrt das Thier durch diese Drehempfindungen über die thatsächlichen Wendungen und Drehungen zu täuschen. Wenn der Vestibularapparat das Thier während der Hinreise orientirte, so müsste durch das Hinzutreten des galvanischen Schwindels diese Orientirung vernichtet werden, oder doch zu einer Orientirungstäuschung führen.

The birds were continuously stimulated all through the forward journey. Upon reaching their destination they were freed from the apparatus and released. The first release was made 54 km. from Vienna. They were all released on April 16, with the following results: (1) the adult control bird (armed with the same apparatus during the forward journey, but not electrically stimulated) was released at 11^h 11^m, reaching Vienna in the early morning of April 18; (2) the young control bird, released at 11^h 16^m, did not return; (3) adult bird electrically stimulated, released 11^h 18^m, reached Vienna on the same day at 1^h 50^m; (4) young bird electrically stimulated, released at 11^h 32^m, did not return.

Many other experiments of this type were made, with essentially the same results. As many birds electrically stimulated returned as normals. Exner next completely anæsthetized with ether several adult and young birds and kept them so during the whole of the forward journey. On reaching their destination the birds were restored and allowed a certain length of time for recovery, and were then released. In the first experiment they were released 43 km. from Vienna. The results were: (1) normal animal, released at 6^h a. m., did not return; (2) anæsthetized animal, released at 6^h 20^m, did not return; (3) anæsthetized animal, released at 6^h 49^m, returned at 11^h; (4) normal animal, released 10^h 21^m, returned at 12^h; (5) young normal animal, released at 10^h 26^m, did not return; (6) old anæsthetized animal, released at 10^h 30^m, returned at 12^h; (7) young anæsthetized animal, released at 10^h 35^m, did not return.

From these experiments of Exner, it is evident that he worked with very poor material, that is, with birds which were poor flyers. Nevertheless, it seems clear from his experiments that electrical stimulation, anæsthetization, etc., during the forward journey have a negative result upon the return of the birds.

INFORMATION ON HOMING PIGEONS GATHERED FROM PRACTICAL FLIERS.

We give below a letter written us by a well-known practical flier in this country, Mr. A. E. Wiedering, for many years race secretary of the Milwaukee district of National Federated Homing Pigeon Fanciers. Mr. Wiedering's letter, while in part touching upon theoretical questions, contains also a large amount of material dealing with the training of the birds.

I was race secretary for several years and took a keen interest in the sport. In the spring of the year we would begin to train the birds over short distances, beginning with 3 or 4 miles, and then 9, 18, 33, 45, 66, and from then on 100, 200, 300, 400, 500, 600, 700, 800, and 1,000 miles. Speaking about training the birds over short distances, we come to a point that has always been under considerable argument with the fanciers. Some claimed short-distance training of great value and others considered it as a waste of time and labor and as entirely unnecessary. I agreed to a certain extent with the latter, as I was of a firm opinion that little is gained by flying the birds over short distances, that their homing instinct, since it is so called, is not developed or improved by it. The only advantage in short distance flying that I could see was that the birds got good opportunities to get used to the shipping baskets and as they felt more at home in them would more readily begin to feed and drink in them, and in that way remain in better condition for their home flights.

After several years of participation in the fancy, I advanced the argument that it was a harder task for a bird to get its bearings towards home from a short distance than from a greater distance. Careful observations of the birds' activities brought me to this belief.

The birds, I believe, home to the locality that they have been used to and not exactly to their loft. I mean that this drawing towards their home location ceases when they get within this locality, and that after that they find their loft through memory of sight by familiar landmarks, etc. I imagine a circle around their home of an imaginary distance of say 5 or 6 miles, and that their homing instinct is not brought into action until they are beyond this locality circle, and if liberated within this circle they have to depend upon their sight memory in order to find their home. I have liberated birds that have had years of experience within 3 and 4 miles of their loft and have seen these birds fly around and around for hours completely at a loss, it seemed, as to their location, and have seen them fly in the opposite direction of their home. Some of these birds had the best of reasons for returning home immediately, such as a hen hatching her egg, or a cock desiring the hen, in which cases they are more eager than usual to return home.

Then again I have been informed by liberators at long distances that the birds had taken the right direction after making a few circles; the best start was made from 1,000 miles, our greatest distance flown, the birds taking the right direction without circling, but starting for home at once. From distances of 100 miles and less the same birds had circled for over 30 minutes and did so repeatedly. From such facts I came to the conclusion that it is easier for a bird to get his bearings from a long distance than from a short one; that this unknown feeling or instinct, or sixth sense, was more intense at a greater distance; and if the bird was liberated in its own locality it did not seem to be in force.

Little, if anything, can be learned of this homing sense by flying these birds; you ship them to a liberator; he liberates them, giving you conditions at the start; you see the birds returning home and know what your local conditions are, but do not know what kind of storms or air-currents the birds have to pass through, and you may have very good results from a flight on one day and from the same distance on another day may have very poor results, although the weather conditions at start and finish may be the same.

I used to stamp my address on flight feathers of the birds and used to hear from them, through parties who would catch them in their own lofts or pick them up dead. In most cases the birds would be found dead without any apparent cause. I believe that they would die from exhaustion.

The speed of a homing pigeon with hardly any winds would average about 1,400 yards per minute for about 100 miles. We have had the birds make it in an average of over 1,900 yards per minute with a strong favorable wind, and 600 yards with contrary winds.

With very favorable weather conditions we have had birds arrive home at 3 p. m. when liberated at 4^h 30^m a. m. from 500 miles, in apparently fresh condition. It may seem strange,

if a bird can make 500 miles in one day why it should not make 1,000 miles in much less than 9 days, but it is supposed that the bird, after his best effort the first day, is exhausted and would look for food, and on account of poor condition, it takes several days before he can resume his journey. I had a bird return home after an absence of 5 years. One year I lost a bird from Walnut Ridge, Arkansas, 500 miles. The next year he happened to come home the very day we were again shipping birds to the same point. I was not very pleased with his performance the year previous and his absence for one year, and therefore shipped him again, feeling that he would surely not be able to return after his long absence and being in very poor condition. You can imagine my surprise when this same fellow was the first bird to reach home, making the distance within 12 hours.

Hobo, the bird that made the world's record from 1,000 miles, came to me when a young bird, quite sick; where he came from I did not know—he just walked into my loft, and being a good-looking bird I allowed him to remain and nursed him to health. He proved to be the most consistent performer I ever owned.

We give below a description of the training of the bird *Hobo*, together with a statement of the flight from Houston to Milwaukee. This note is of interest in view of the fact that it shows the training stages which are usually given a bird before long flights are expected of it:

A week ago last Wednesday [July 24, 1901] a number of birds owned by members of the Milwaukee district of National Federated Homing Pigeon Fanciers, were liberated at Houston, Texas, an air-line distance of 1,000 miles. Up to to-day two birds have arrived, the first to come home being the bird *Hobo*, owned by A. E. Wiedering, which arrived Saturday, and the second being from the loft of C. G. Loeber, *Little Hen*, which arrived yesterday, making the trip in 14 days 1 hour and 19 minutes. They are the first two Milwaukee birds ever to cover the distance, and the winner, *Hobo*, did it within but a few hours of the world's record. Several of the descendants of *Hobo* are flying in the service of *The Journal*. *Hobo* is a strongly built blue-checked cock and is now in his prime, being only 4 years old. He will not be flown again, as his owner thinks that he has done his duty. He has always been a reliable homer, but has hardly ever shown any speed in distances less than 600 miles. In 1899 he flew one race from Shabbona, Ill., at a rate of 50 miles an hour. In 1900 he won fourth place in a race from Little Rock, Ark., air line, 617 miles; 33 birds competing. He arrived at 5^h 30^m a. m. on the third day after liberation. This year he won second place from the same distance, also making it on the third day; 47 birds competing, but only two making it in the time limit for that distance, which is 3 days. After a week's rest he was shipped with 25 others to Houston, Texas, air-line distance, 1,000 miles. After being in the shipping basket 17 days, the 26 birds were liberated on July 24 at noon, all the birds taking their course immediately. *Hobo* arrived August 3, at 8^h 15^m a. m., taking 9 days 20 hours and 15 minutes to cover the distance.

Following is his complete record:

1898.

9 miles.
18 miles.
33 miles.
45 miles.
66 miles.
100 miles, Shabbona, Ill., to Milwaukee.
200 miles, Delavan, Ill., to Milwaukee.
400 miles, Ironton, Mo., to Milwaukee.

1899.

33 miles.
66 miles.
100 miles, Shabboth, Ill., to Milwaukee.
200 miles, Delavan, Ill., to Milwaukee.
300 miles, Brighton, Ill., to Milwaukee.
400 miles, Ironton, Mo., to Milwaukee.

1900.

33 miles.
45 miles.
66 miles.
100 miles.
200 miles.
400 miles.
617 miles, Little Rock, Ark., to Milwaukee.

1901.

18 miles.
45 miles.
100 miles.
200 miles.
300 miles.
400 miles.
617 miles.
1,000 miles, Houston, Texas, to Milwaukee.

Little Hen is 5 years old and has been in races for the last 4 years, and, although she has never made any phenomenal time, she has always been a reliable homer. This year she homed from a 500-mile station twice, and the amount of experience she has had on the road evidently stood her in good stead in this race, as the passage through the drought-stricken country was a severe one.

Since 1901 *Hobo's* record has been beaten many times. Many of the birds holding or having held world's records have belonged to lofts in Fort Wayne, Indiana. The fanciers in this city have succeeded in breeding a strain which is unrivaled in the history of homing. It would be interesting to determine whether the location with respect to winds, etc., has anything to do with the records these birds have made. The difference of course may lie wholly in the strain of bird which has been obtained. We give below the letter of Mr. O. W. Anderson, of that city. It concerns the record of the present world's champion, *Bullet D-1872.*

This bird was hatched March 20, 1909, and when about 4½ months old began training, flying the training stations of 2, 5, 8, 15, 25, 40, and 75 miles and the young bird races of 100 and 200 miles; this bird being my first one in the 200-mile race. In 1910, as a yearling, she flew the training stations from 2 to 75 miles and on four successive Sundays flew the 200, 300, 400, and 500 mile races, arriving from the 500-mile race on second morning and the first three races the same day, being my first bird in all these races.

In 1911 she flew training stations mentioned before; the 200, 300, and 500 mile races, arriving the same day in 200-mile race and the second day in 300 and 500 mile races, the weather conditions being unfavorable in these last two races.

In 1912 she flew the training stations and 200, 300, and 500 mile races, arriving the same day from 200 and 300 mile races and fourth day from 500-mile race, but weather was extremely bad for this last race.

In 1913 she flew the training stations and was first bird to my loft from 200-mile race, arriving on third day at 6 a. m., the weather conditions being very unfavorable on account of a very severe hail storm. She was also my first bird from 500-mile race, being liberated at 4^h 30^m a. m. at Springfield, Missouri, and homed at 3^h 26^m p. m. same day. Three weeks later sent her to Abilene, Texas, 1,010 miles (air-line measure) from here, birds being liberated at 4^h 30^m a. m. on July 11, 1913, and this one homed at 4 p. m. on July 12; flying time, 1 day 11 hours 30 minutes and 6 seconds. Needless to say, the weather conditions were ideal, a strong wind assisting the bird materially. In this same race a bird belonging to Dr. John Schilling of this city homed at 11^h 30^m a. m. the following morning (July 13) and a third bird was received by Mr. F. Nahrwald of this city a half hour later. The birds in this race were liberated by express agent at Abilene, Texas, as per telegram sent Racing Club here on same date, and liberating report bearing names of witnesses on file at office of club here.

All the above races were in the Old Fort Homing Club of this city and were flown under rules of American Racing Pigeon Union. The best previous record for 1,000 miles was made by a pigeon belonging to Mr. H. Beech of this city in 1912, the time being 2 days 9 hours and some odd minutes, and this record lowered the time made by a bird belonging to Mr. L. Gebfert, of this city, in 1910, this time being 3 days 11 hours and some odd minutes.

Have not entered my pigeon in any subsequent race, but am saving it for its record's sake, as there is of course a certain risk of loss of birds in any race.

From these startling records it must not be supposed that very many birds, flying from an average loft, return from 1,000 miles unless they have been previously trained over intermediate distances very completely. A few years ago over 100 homers belonging in Baltimore were released at Key West, Florida. Not one ever returned. All had been trained at least 500 to 700 miles from Baltimore in the direction of Key West.

THE RECORD OF A TYPICAL FLIGHT.

Thauziès* gives the record in detail of homing pigeons released by him at Geneva when the Sixth International Congress for Psychology was in session. The birds belonged to three cities, Versailles, Guéret, and Gannat; none of

*Thauziès, A.: Expérience d'orientation lointaine, Archives de psychologie, IX, 1910, p. 66.

them had flown from Geneva previously. The Versailles birds had been trained toward Brest, the Guéret birds towards Amsterdam. Four of the birds from Gannat had already been trained from Macon to Gannat. He says nothing of the training of the other four Gannat birds.

Versailles birds: 24 birds were released in Geneva at 7^h 10^m August 6; 2 returned the same day at approximately 5^h 45^m p. m.; 9 returned August 7 at different hours; by August 10 all had returned.

Guéret birds: 34 birds were released in Geneva at 7^h 15^m on the morning of August 6; 2 returned the same day a little after noon. The rest returned on the following morning.

Gannat birds: 8 pigeons were released in Geneva at 7^h 20^m a. m., August 6. None returned the first day; 3 returned on the morning of August 7; 1 on the 8th; 1 on the 9th; 1 on the 11th, and 1 on the 12th. On August 17, 1 bird was still out.

We give this record of pigeons in full because it brings out one point very clearly, viz., that even in short flights all of the birds do not return promptly. Those we read of in the homing-pigeon records are usually of the first bird in the loft to return and it is rare that all of the birds from any given loft return on the first day, even when the distances are short. The above distances were all very much less than the longer flights made from Bird Key. The distance from Geneva to Versailles is 280 miles, to Guéret 220 miles, and to Gannat 150 miles.

DATA ON THE SPEED OF THE HOMING PIGEON.

The following record was furnished us by Mr. Wiedering. It relates to average speed in a 200-mile contest.

161 birds were shipped on Saturday to Delavan, Ill., being a little over 200 miles on an air-line. The birds were liberated on Sunday at 7^h 10^m a. m. by W. M. Cornett, agent of the Chicago & Alton Railroad Company, who reported weather fair and wind from the southeast. At home the weather was very cloudy and the wind from the south, changing to east about an hour before the arrival of the first bird. According to reports local showers prevailed along the greater portion of the course and the speed made is very satisfactory, under the conditions. The first bird timed at the several lofts and the speed per minute attained by each is as follows:

Alex. E. Wiedering, 1,135.30 yards.
H. J. Baumgaertner, 1,134.61 yards.
Aug. Bergenthal, 1,134.59 yards.
John Radke, 1,127.99 yards.
William R. Knell, 1,115.71 yards.
Edw. J. Phipps, 1,081.48 yards.
Val. Burkard, sr., 1,067.99 yards.
Peter J. Ries, 1,003.19 yards.

When the distance is shorter the average speed, with other conditions equal, usually rises. From the same source we have obtained a record for a 100-mile flight:

The Milwaukee district of the National Federation of American Homing Pigeon Fanciers conducted its 100-mile race from Shabbona, Ill., on Tuesday, May 31. 161 birds were shipped by Adams Express on Saturday of last week. The weather being unfavorable on Sunday and also on Monday, liberation did not take place until the date above mentioned.

A. J. Harton, agent of the Chicago, Burlington & Quincy Railway, acted as liberator, and reported that the birds were liberated at 6^h 30^m a. m., with clear weather and southeast wind. At home the weather was also clear, with wind changing from southeast to southwest.

A bird from the loft of H. J. Baumgaertner was the winner of the race, and covered the distance of 101.27 miles in 2 hours and 43 seconds, being at the rate of 50.30 miles an hour.

The first bird timed at each of the respective lofts, with the average speed per minute, is as follows:

H. J. Baumgaertner, 1,475.60 yards.
William R. Knell, 1,471.29 yards
A. E Wiedering, 1,452.89 yards.
Aug. Bergenthal, 1,411.29 yards.
Edw. J. Phipps, 1,395.07 yards
P. J. Ries, 1,373.69 yards.
Val. Burkard, sr., 1,349.08 yards.

The above agrees closely with Ziegler's* account of the flights of German birds. According to this author the direction and velocity of the wind greatly influence the flight of the birds. In flights from great distances (100 to 600 km.) the average speed of the best birds with no wind is 1,100 to 1,150 meters per minute. In a favorable wind the speed rises to 1,300 to 1,600 meters, and occasionally to 1,600 to 1,950 meters. In unfavorable winds the speed of the best birds varies from 500 to 800 meters, according to the strength of the wind. The pigeons never fly to great heights—never over 1,000 to 2,000 meters and usually much lower.

THE PROBLEM OF HOMING IN BIRD MIGRATION.

In this paper, as has been stated, we shall not discuss the general features of bird migration. Nevertheless, two or three migrations deserve mention in this connection because it is clear that the problems involved in them are the same as those involved in the general study of homing. Cooke,† after showing that many of the birds fly by night as well as by day, takes up some of the longer routes of the birds. The longest continuous flight in the world is possibly 2,400 miles. The golden plover nests along the Arctic coast of North America and when the young have become independent the fall migration begins. The birds fly to the Labrador coast and remain there for several weeks, where food is abundant. After leaving Nova Scotia the birds fly due south to the coast of South America. The golden plover takes a straight course across the ocean and, if the weather is propitious, makes the whole 2,400 miles without pause or rest. But if tempests arise, it may be blown out of its course to the New England coast and start anew on the advent of fair weather; or it may rest for a few days at the Bermudas, one-third of the way along its course, or at the nearest of the Lesser Antilles, still 600 miles from the mainland of South America. These, however, are emergency stop-overs, to be resorted to only in case of storms. Having accomplished its ocean voyage, it passes across eastern South America to its winter home in Argentina. After dwelling 6 months in Argentina it finds its way back to the Arctic regions by an entirely different route. It travels across northwestern South America and the Gulf of Mexico, reaching the United States along the coast of Louisiana and Texas; thence it travels slowly up the Mississippi Valley and by early June is again at the nesting site on the Arctic coast.

This same bird dwells also in the Hawaiian Islands and travels from that point to Alaska, spending the winter in Hawaii and nesting in the spring in Alaska. The Hawaiian Islands are 2,000 miles from the California coast on the east, 2,400 from Alaska on the north, and 3,700 miles from Japan to the west. This is a very wonderful route, since between Hawaii and Alaska there are no landmarks.

Surprising as are these results from the golden plover, they are surpassed by the exploits of the Arctic tern.‡ This bird dwells alternately in the Arctic

*Zoologische Jahrbücher, 1897, x, p. 238.

†A summary of his work appears in the National Geographic Magazine, 1911, p. 346.

‡According to Cooke, the Arctic tern arrives in the Arctic regions about June 15, and leaves August 25, spending thus 14 weeks at the nesting site. "They probably spend a few weeks longer in the winter than in the summer home; and if so, this leaves them scarcely over 20 weeks for the round trip of 22,000 miles. Not less than 150 miles in a straight line must be their daily task, and this is undoubtedly multiplied several times by their zigzag twistings and turnings in pursuit of food."

and then in the Antarctic region. Their track over the 11,000 miles separating these regions is known to no one. Scattered individuals have been noticed along the United States coast south to Long Island, but naturalists have never observed these birds during migration, nor can they construct accurately the routes of the birds from the records in hand. This means that the birds probably choose an ocean route and that their flight is almost continuous.

Several interesting points bearing upon the problem of homing come out from these studies of Cooke and others: first, that the plover and the Arctic tern have no hesitancy in undertaking ocean routes several thousand miles in length which can offer few if any landmarks in the ordinary acceptation of that term; secondly, that the birds must fly by night, which again lessens the probability that landmarks of a visual kind are used.

HOMING IN THE FRIGATE BIRD.

In regard to the homing ability of the frigate bird, the following interesting observation is cited from the *Zoological Society Bulletin* No. 29, p. 421. The article is unsigned, but was written (so Dr. Chapman informs me) by Dr. Charles H. Townsend.

In the course of a winter's voyage on the *Albatross*, in the South Seas, the writer found, among the natives of the Low Archipelago, many tame frigate birds. The latter were observed on horizontal perches near the houses and were supposed to be merely the pets of the children who fed them. They were entirely tame, having been reared in captivity from the nest. As our acquaintance with the people developed, we discovered that the birds were used by them after the manner of homing "pigeons," to carry messages among the islands.

The numerous islands of the Low Archipelago extend for more than 1,000 miles in a northwest and southeast direction, and it appears that the birds return promptly when liberated from quite distant islands. They are distributed by being put aboard small vessels trading among the islands. The birds are liberated whenever there is news to be carried, returning to their perches sometimes in an hour or less, from islands just below the horizon and out of sight of the home base. Generally they are in no great hurry. As the food of the frigate bird may be picked up almost anywhere at sea, there is no means of ascertaining how much time the bird loses in feeding or trying to feed *en route*. It may also linger to enjoy its liberty with other frigate birds.

I did not observe tame frigate birds elsewhere in Polynesia, but Mr. Louis Becke, who is familiar with most of the South Sea Islands, says they were used as letter carriers on the Samoan Islands, when there in 1882, carrying messages between islands 60 to 80 miles apart. When he lived on Nanomaga, one of these islands, he exchanged two tame frigate birds with a trader living on Nuitao, 60 miles distant, for a tame pair reared on that island. The four birds at liberty frequently passed from one island to the other, on their own account, all going together on visits to each other's homes, where they were fed by the natives on their old perches. Mr. Becke's pair usually returned to him within 24 to 36 hours. He tested the speed of the "frigate" by sending one of his birds by vessel to Nuitao, where it was liberated with a message at $4^h\ 30^m$ p. m. Before 6 o'clock of the same day the bird was back on its own perch at Nanomaga, accompanied by two of the Nuitao birds, which, not being at their perch on that island when it was liberated, it had evidently picked up *en route*.

Sixty miles in $1\frac{1}{2}$ hours is probably easy enough for the frigate bird, as in Malayo-Polynesia it is said to have frequently returned a distance of 60 miles in one hour. It becomes entirely tame and familiar when raised from the nest, and if given liberty returns regularly to its home-perch at night. The largest rookery of frigate birds I have seen is at Tokokoto, in the Low Archipelago.

Frigate birds inhabit the tropical and subtropical seas. The spread of wing is phenomenal for the size of the bird, being about 8 feet, giving wing-power perhaps unequaled.

On page 48 our own experiments on homing in the frigate bird will be considered.

HOMING IN OTHER VERTEBRATES.

Of homing in mammals little is known. We have a series of anecdotes in the literature containing probably somewhat more than a grain of truth.* We give below some of the material that has been sent to us personally from "animal lovers" over the country. We give these records for what they are worth.

The following incidents have been related concerning mammals:

(1) In May I sent my coach-dog (3 years of age) to Northampton, Massachusetts, 50 miles away. He went by carriage with my wife to Brattleboro, Vermont, 12 miles distant, and from there they took the train at $8^h\ 30^m$ p. m., in a hard rain. A closed carriage took them from the station in Northampton, where they arrived at 11 p. m. Saturday. On Monday at about 3 p. m. the dog disappeared and on Thursday about 3 p. m. he walked into our house, having been on the road 3 days. The last 12 miles he has covered a good many times, as he always goes when I drive. He evidently covered more than 50 miles, for he was seen on Tuesday near Amherst, which is not on the direct route.

(2) [A correspondent] raised a dog, crossed with hound and pointer, and littered in Lawrence. When a year old he took the young dog to Boston, got on board of a sailing-vessel, went by sea and river to Bangor, Maine, drove 40 miles into the woods at Cleveland's Camp, and hunted there two weeks, the dog proving to be a great success for quick, fast runs and returns to camp. After the hunting was over, and while on his back trip to Bangor, the dog jumped from the wagon into the bushes, having heard or smelled a deer, and went off on a hot chase. The boats ran only once in two weeks, so that, much as he valued the dog, it was necessary to go on. He took the boat at Bangor, returned by river and sea to Boston and back to Lawrence. About 2 weeks afterward the dog crawled into his yard, footsore and half starved, but safe at home and glad to get back.

(3) Some 16 or 18 years ago a young man brought to this place from his, and their, former home in Indiana two dogs of the greyhound type. Not long after they arrived here they disappeared, and in 6 weeks or so one of them arrived at his former home in Indiana, and a couple of weeks later the other one got back there. The distance is over 700 miles.

(4) I spent my vacation the past summer at my mother's, 3 miles from Siler City, North Carolina. My brother, who lived at Siler City, had a 3-months-old calf which he wanted to pasture at my mother's farm. Accordingly the calf was brought along the road from the town. The next day the animal got out of the open gate and returned home. I followed its trail—it had recently rained. The calf first took almost a bee-line for its home; crossed a small ditch, then came a large ditch, which it wandered down some distance, but returned and crossed near its direct line. This was at a distance of a quarter of a mile from the road by which it had been delivered, and all the space is covered by thick forest. When the calf struck the main road it proceeded along this to its home. This animal never had been out of its lot until it was brought to my mother's, and yet its sense of direction was so accurate that it took a straight line for home until it reached the road by which it had been brought. Then it depended upon its memory of the road, although it might have followed a path in a much more direct line.

One of my mother's neighbors, shortly after the war, moved into the State of Tennessee, 500 miles away. A dog was taken along. One day the animal was missing, and, a few weeks after, it turned up at its old home.

(5) I have a collie a little more than 2 years old. We took him with us to our camp at Point au Baril, Canada. The dog had lived in Avondale, a city suburb, all his life, and knew absolutely nothing of anything more than the ordinary suburban streets and the neighboring country fields. Our camp is on an island in the midst of a great archipelago. Every summer the people who have gone with us to the camp lose their way because the islands so closely resemble one another and are so bewildering. The dog made himself perfectly at home on our island, and although he had never swum before, he now learned

*Observations on the homing of animals similar to those above may be found in the Revue Scientifique, 1897, made by Dusolier, p. 759, and by Artault, p. 793. Scattered through the Revue Scientifique one finds a large number of such anecdotes.

and soon developed a passion for riding in canoes, rowboats, and the steam-launch. However, he never was taken outside of the island and had possibly been 500 yards away, but not farther.

One day we took him on an excursion in a launch, some 6 miles from the camp. He was not able to see the island as we passed, because he was down in the bottom of the boat. On the return trip we got out at a little store about 2½ miles from the camp by a direct water route. Some of our boys were in a canoe at the dock, and the dog jumped into the water and tried to follow them. They did not see him, nor did we know of this until later. It was dusk, and after searching for him in vain we returned to the camp. He is a valuable dog, much beloved by all of us, and after dinner that evening we started out in canoes, went back to the store, and searched everywhere for him. We had heard that a dog (it must have been ours) had been seen swimming to the rock on which was a range light, some three-quarters of a mile from the point, but no trace was found of him there. At 11 o'clock that night the dog reappeared at the store, put his nose in at the door, and when they tried to call him he seemed frightened and disappeared in the darkness. There is a way by land from the store to our camp—6 miles of unbroken primeval country, burned-over forest with tangled swamps every quarter of a mile. It is an almost impassable territory where even the Indians get lost.

The next morning, while we were preparing breakfast, the dog appeared swimming down the channel. Our Indians insisted that he had spent the night swimming from island to island, and that when daylight came he smelled our camp—the smoke of the kitchen fire and the odor of the breakfast being prepared. At all events he made his way home, and if he swam directly from the Point it was 2½ miles by water, but if he swam from island to island it must have been a much greater distance. The channel is not direct and open, but winds in and out, and he had no knowledge of the way, so that I do not believe it is possible that he could have been guided by any landmarks.

We have taken pains to write to the contributors of each of these stories and to obtain what supplementary data we could. The incidents seem actually to have happened, but none of the contributors could state with accuracy facts enough to enable us to use the material in a scientific way.

Last summer a similar incident, with not so favorable an outcome, happened to one of the writers at his summer cottage on Stony Lake, Ontario, Canada. An 18-months-old pure-bred cocker spaniel went from the cottage to the store a mile away in the canoe with a member of the household. The store is situated on a large island containing several hundred acres. In the hurry of leaving the store the dog was left behind. After luncheon we set out to find him, first going to the store. The dog had disappeared. We then began to paddle around the island, calling the dog at every moment. We had just about given up hopes of finding him when we happened to glance at one of the small islands to the southwest of the island on which the store is situated, and in a direction almost opposite from that in which our cottage lay. There we saw the dog standing forlornly on the shore. A new house was under construction on this island and the carpenter told us that a short time before the dog had left the main island and had swum to his place. The distance from the store to the point where we found him was probably a mile and a quarter. It is interesting to note that our own cottage was under course of construction, and the fact that the dog finally landed at a place where hammering was going on might indicate that it was the sound of the hammering which made him take the direction which he did take. He probably wandered all over the large island and at some time in the course of his wandering the sound of hammering may have become audible.

Both Hodge and Fabre* give data bearing upon orientation in cats. The following account is summarized from Hodge.†

A Maltese tom cat was taken out in a row boat on one of the large Wisconsin lakes on a sultry, extremely dark night. The boat was rowed due north straight out towards the middle of the lake, which is 2 miles in width. At first the cat was quiet and then he began to get restless and extremely anxious to get home. He would climb to the end of the boat,

*Souv. ent., II, p. 124. †Popular Science Monthly, 44, p. 758.

stretch his head towards home, and mew continuously. The boat was turned round and round to try to throw Tom off his bearings, but to no effect. Whether right side or left, bow or stern, Tom was always on the part of the boat nearest home and straining as far as he could in that direction. Fully a mile from any shore how could he tell which shore was which? But few lights were visible and the lake is thickly wooded, and the cottages stand well back among the trees. No member of the party, except the experimenter, who used the north star for guidance, maintained orientation. Tom was wrapped up and held flat in the bottom of the boat, and then released. This likewise made no difference. He would go immediately to the part of the boat nearest home. Members of the party were blindfolded and required to guess whether the boat was turned or allowed to stand, or whether rowed in a straight line or a circle, and it was an even chance whether they guessed right or wrong. No air was stirring and no odors were detectable to the human being.

Hodge leaves the explanation in doubt, but is inclined to attribute Tom's orientation to hearing. In support of this suggestion he cites an incident connected with deer-hunting in Montana:

While lying on the top of a hill he watched a doe as she kept guard over her two fawns. They were fully a half mile from where the observer lay. He focused his field-glasses on the group and became much interested in the alertness of the doe. "At a slight noise, occasioned by my change of position [he was of course out of sight of the deer], a noise not even noticed by myself, I was surprised to see the doe start, turn around, and point her ears in my direction. After a few minutes silence her attention was directed elsewhere, and this time I made the least "ahem"; again both ears and head were directed towards me, and so in turn for an hour I tried all manner of slight sounds—low whistles, snapping my fingers, tapping my rifle stock, scraping the ground with my foot; all were followed with the precision of response of the strychnized frog with the attent turning of the ears in my direction.

If the movements of the deer were really made in response to auditory stimuli, we would have to believe that the deer has an almost unlimited auditory acuity. It does not seem to be quite excluded, however, that the deer might have been reacting to visual stimuli, possibly through shadows, or occasioned directly by the movements of the observer. Dr. Hodge, however, does not profess to maintain that his experiment was a crucial one.

Victor Franz* maintains that fish have an extraordinarily well-developed sensitivity for detecting differences in hydrographic conditions. Currents are detected in the water by means of the lateral-line organs, temperature through warm and cold corpuscles in the skin (or through homologs), salinity through the sense of taste, depth through pressure. But Franz's paper is a *résumé* of convictions rather than a report of experiments. He argues that the spawning migration is not actuated by sexual instincts, but that it is an adaptive change which has as its end result the obtaining of optimal developmental conditions for the young. It is conditioned solely upon hydrographic phenomena.

From the above survey of the literature it would seem impossible to get any exact delimitations of the problems involved in homing. The work on the homing pigeon has been carried out in too desultory a fashion and too much under the influence of particular theories to afford satisfactory material for hypothesis. Crucial experiments designed to bring out the facts as to what the *untrained* homing pigeon can do are lacking. Until such have been made speculation on the mechanism of return is useless. In so far as any conclusions at all may be drawn from the experimental work on the homing pigeon, we

*Die Laichwanderung der Fische, Archiv für Rassen und Gesellschaftsbiologie, 7, 1910, pp. 159–168.

may say with Exner that nothing which is done in the way of controlling the behavior of a bird (or its temporary physiological condition) on the forward journey affects its ability to return to its côte.

Our own work in Tortugas has been confined largely to defining the problem of homing. We have had there an unrivaled opportunity to test the ability of *untrained* birds to home in a territory through which they have never passed, and over open-water stretches many hundreds of miles in length which apparently can offer no "landmarks." A brief account of the instinctive life of the noddy and sooty tern precedes the description of our own experimental work upon homing.

FIG. 1.—Geographical situation of Tortugas. Note the absence of land between Tortugas and Galveston

HISTORY OF THE TORTUGAS NODDY AND SOOTY TERN COLONIES.

The noddy tern (*Anous stolidus*) and the sooty tern (*Sterna fuliginosa*) are well known in tropical and juxtatropical waters. The colony which is to be found in the Tortugas from May to September was described by Audubon in 1837. He states that at the beginning of May the noddies collect from all parts of the Gulf of Mexico for the purpose of returning to one of their breeding places on one of the Tortugas called "Noddy Key." *They nearly equal in number the sooty terns, which also breed on an island a few miles distant.* He later describes the sooty tern as nesting on Bird Key. In regard to the number of sooties he says:

> As the chain grated the ear I saw a cloud-like mass arise from Bird Key, from which we were only a few hundred yards distant, and in a few minutes the yawl was carrying myself and my assistant ashore. On landing I felt as if the birds would raise me from the ground, so thick were they all around and so quick the motion of their wings. Their cries were indeed deafening, and yet not more than half of them took to their wings on our arrival, those which arose being chiefly male birds, as we afterwards ascertained. We ran across the naked beach and as we entered the thick cover before us and spread in different directions we might at every turn have caught a sitting bird or one scrambling through the bushes to escape us.

Since Audubon's time the islands have changed considerably in form. Only noddy and sooty terns (with a few least terns, which breed upon Loggerhead) breed in the Tortugas, and these appear only upon Bird Key. Furthermore, in 1907, there were 13 times as many sooties as noddies. This island is now somewhat triangular in shape and has an area of about 6,000 square yards (see plate 1; also fig. 2, p. 62). It is covered with bay-cedar bushes, cactus, and various low-growing vines (plate 2). It stands about 6 feet above mean tide level. Plate 1 gives the general appearance of the island at low tide. The small buildings were formerly used in the hospital service of Fort Jefferson. They are now used by the warden. Northeast of Bird Key, about $1\frac{1}{4}$ statute miles distant, stands the now deserted Fort Jefferson (Garden Key). In this fort is to be found a reef light about 50 feet in height. To the west of Bird Key lies Loggerhead Key, about 3 statute miles distant. This is the largest island in the group. Upon it is to be found a first-class light-house, with revolving light 151 feet in height. Certain other small islands and reefs are to be found near Bird Key. The deep waters of the Gulf of Mexico surround the group as a whole. Eastward of Bird Key, about 65.8 statute miles distant, lies Key West. Between Bird Key and the environs of Key West are found in order Rebecca Shoals lighthouse and the small islands, Marquesas and Boca Grande. On the westward side one finds only the waters of the Gulf until the shore-line of Texas is reached, Galveston being 855 statute miles distant. This strip of open water proves a magnificent route for homing experiments. Figure 1 shows the topographical relations in the Gulf region.

RÉSUMÉ OF INSTINCTIVE LIFE OF COLONIES.

In our previous paper (Watson, J. B., *opus cit.*) we gave a detailed account of the nesting behavior of the noddy and sooty. In this connection, we wish simply to supply a summary of certain of our observations bearing upon the instincts and habits which are connected directly with homing or with the control of experiments upon homing. A large amount of new material has

been incorporated. This summary will deal (1) with the migration of the birds; (2) with the food and feeding instincts; (3) nesting activities; (4) swimming activities; (5) retention of nesting habits.

MIGRATION.

The time of the northern migration apparently is variable. Some statistics have been made of the approximate time of arrival at Bird Key by the warden of the Department of Agriculture and the Audubon Societies. This warden, since Tortugas was set aside as a game reserve, is present throughout the breeding season. Through the kindness of Dr. T. S. Palmer we are able to give, in table 2, the data of the time of arrival at Bird Key. It will be seen that the time of arrival is variable. The birds appear upon the island rather suddenly, noddies and sooties coming together in large flocks. They come in usually from the south and southwest. Their route is unknown. It is presumed, however, that the group leaves the Caribbean Sea and comes to the southern shore of Cuba, and then takes a northwesterly route to Tortugas.

TABLE 2.

Year.	Date of arrival.
1907	Apr. 27
1909	
1910	Apr. 27
1911	Apr. 14–May 6
1912	Mar. 20
1913	Apr. 8

The southern migration begins some time in the latter part of August and early part of September. Dr. Palmer furnishes us the dates given in table 3.

Again we see that the date for the southern migration is variable.* The birds apparently leave in flocks in a southwesterly direction. It would be interesting to know if the old birds leave first, deserting the immature birds. This seems quite improbable in view of the fact that the eggs are laid during about 4 to 5 weeks in May and early June. This means, of course, that the young birds mature from about the middle of August to the middle of September. The birds laying first would be ready to leave first with their young. Many anecdotes are told about this colony and others to the effect that the young are left by the parents and that an "enormous pilot bird" appears always in time to lead the young to their distant and unknown winter goal.

TABLE 3.

Year.	Date of departure.
1907	Sept. 22
1909	Aug. 15–27
1910	
1911	Aug. 9–Sept. 25
1912	Aug. 28–Sept. 20
1913	Aug. 29–Sept. 12

*The impulse to migrate does not concern us in the present paper. It seems worth mentioning, however, that neither temperature nor food conditions (?) vary much from August 20 to September 20. Dr. Mayer furnished us the accompanying table of temperatures (given in degrees Fahrenheit) covering the period of August 9 to September 27. It will be seen that the average temperature is practically constant during the entire period of migration. These records tend to support the commonly accepted view that the stimulus to migration is an intraorganic one. (See Chapman, Birds of Eastern North America, p. 56.)

Date.	High.	Low.	Aver.	Extreme range for week.
1912				
Aug. 9 to Aug. 16	87.5	84	86	3.5
16 23	87	81.5	85	5.5
23 30	87.5	83	86	4.5
30 Sept. 6	98.5	84	87.5	5.5
Sept. 6 13	88	81.5	86	6.5
13 20	87.5	81	84	5.5
20 27	87	77	84	10

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NODDY TERNS.

Fig. 1. The peculiar bowing or nodding reaction in the noddy.

Fig. 2. A typical nest with male and female in possession.

FEEDING INSTINCTS.

The noddy and sooty live almost exclusively upon minnows during the breeding-season. During the remainder of the year nothing is known of their diet. But since captive birds learn to eat cut fish with the greatest difficulty, we judge that their food, even during the winter months, is live minnows. The minnows are obtained in the following way: when large fish begin to prey upon the schools of minnows, the minnows, in their efforts to escape, spring out of the water; the birds, always hovering over the surface of the water, flock to these places and catch the minnows just as they leave the water. The following note was made on one of our trips around the island:

June 15, 1910: Watched birds feeding about 2 miles out. The group comprised about 50 birds, mostly noddies. Some sooties were present and 6 least terns. The noddies hovered about 12 inches over the water, following the school of minnows. They make a rattling noise, all the while, audible 200 feet away. The noise made by any individual bird gets stronger as the fish is sighted. The noddy steps on the water often, sometimes strikes it with the breast, not in diving movements, but in those of pursuit. Never folds the wings and sits in the water.* The sooty behaves in a very similar manner, except that I saw one bird apparently diving for a fish; it certainly partially submerged its body. The least tern fishes in a characteristic way: It hovers about 6 to 10 feet above the water. The moment the fish is sighted, it folds its wings in a V shape, and shoots vertically downward, striking the water head first and going under. It does not always swallow the fish, but comes up with it in its beak. No resting on water, even with least tern; dives in an incredibly short time; comes up and is ready to strike again.

It is obvious that the tern, in order to obtain food on long trips, must be constantly in an environment both where large schools of minnows abound and where schools of large fish are constantly attacking these minnows. Furthermore, the minnow must be of a type which will spring out from the water in its efforts to escape. These peculiar conditions can be found in May (the one month when homing can be conducted) probably only in tropical and juxtatropical waters. This fact probably will always make a flight from New York, *e. g.*, impossible. On the other hand, the line from Bird Key to Galveston (Galveston being more nearly in the latitude of Bird Key) probably offers favorable feeding conditions throughout its length.

In regard to the distance from Bird Key covered by birds in their daily search for food, our 1910, 1912, and 1913 observations abundantly confirm our early (1907) observation that this distance rarely exceeds 17 to 18 miles. It may be said in passing that these birds are practically unknown both in Key West and in Galveston. Furthermore, in the many trips which we have made between Key West and New York we have never noted these birds north of Key West; hence we must conclude that Bird Key (plus 17 to 18 miles) is the northern limit of migration for both noddy and sooty.†

*In the 1907 paper we had never seen noddies and sooties swimming in the water. In 1910 we saw one noddy fly into the water, fold its wings, and begin swimming like a duck. About 10 other noddies joined this one. This is the only occasion upon which we have ever witnessed swimming movements. We have never seen the sooty in the water, except when accident has overtaken it.

†They are reported sporadically along the eastern coast of North America. Such birds in all probability have been blown northward by heavy winds. This occurs rather frequently in the migrating period.

NESTING ACTIVITIES OF NODDIES.

The noddy tern builds its nest usually in the bay-cedar bushes, of whatever material it can find; small twigs (plate 4, fig. 1), seaweed, and shells are all used. At times the nest is placed on low-growing grasses (plate 3, fig. 2) and cacti, rarely on the bare ground. Both birds work upon the nest. Usually several days are required to complete the structure. One egg is laid. The period of incubation is 28 days. If the egg is removed, usually another egg is laid. In this way it becomes possible to prolong to some extent the breeding period.

While the nest is under construction and during the period between the completion of the nest and the depositing of the egg, the male feeds the female (plate 4, fig. 2). During all this time the birds are wild and beyond easy means of control. As soon as the egg is laid the instinctive cycle changes and the birds no longer fly out at the approach of the experimenter. At this time the noddy becomes a suitable subject for experiment on homing. At the beginning of the brooding season proper, still another marked change comes in the feeding instincts. Both birds brood the egg, shifting more or less regularly at 2 to 5 hour intervals. At night both birds are invariably present.

NESTING ACTIVITIES OF SOOTIES.

The sooty tern usually scratches out a shallow depression in the sand (plate 4, fig. 3). Since 1907 the island has become covered with a dense undergrowth and bare stretches of sand are not common. The vast majority of the eggs are now laid bare upon the earth or upon a leaf carpet. It is advisable, where homing experiments are to be carried out, to clear out this undergrowth in desirable areas. Observation of the nest is thus made easy. Clearing away the undergrowth has one disadvantage by reason of the fact that in the last few years the laughing gulls have become quite numerous on Bird Key. They persistently rob both the noddy and the sooty nests. Some means must be found for preventing their depredations. They were quite troublesome in 1913.

The sooties, like the noddies, are wild until the egg is laid (in certain nests two eggs are laid). During the brooding period they become quite bold and are easily caught. The brooding period is about 31 days. Male and female take regular turns at the nest. Shifts occur only once in 24 hours, however, and at times only once in every 48 hours. For this reason it is always more difficult in homing experiments to determine the time of arrival of the sooty at night than that of the noddy. What becomes of the sooty when not on the nest is difficult to say. The two occupants of a nest are rarely seen together. We are inclined to think, though, that both birds are near the nest at night. Close observation of the sooty is difficult unless the bird is marked. There are probably more than 18,000 (possibly 30,000) sooties on Bird Key, and the nests are often separated by a distance not greater than the length of the bird (plate 4, fig. 3). Notwithstanding this crowded condition each nest is accurately localized by its proprietors. Encroachments upon a nest or its environs by a neighboring bird are fiercely punished by the owners. The nests are carefully maintained all during the brooding period. When the young bird is 10 to 12 days of age there is a gradual desertion. The young bird runs to meet the parent and begins to feed at whatever place the meeting is effected.

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INSTINCTIVE ACTIVITIES OF NODDIES AND SOOTIES.

Fig. 1. A noddy gathering bay cedar branches. Fig. 2. Male noddy feeding female.

Fig. 3. Group relation of sooty nests.

SWIMMING AND RESTING UPON THE WATER.

In an earlier paper we called attention to the fact that the birds during the nesting season, even while feeding upon minnows over the surface of the water, rarely come to rest in the water, and that neither the noddy nor the sooty is ever seen swimming in the water. Since, in the experiments on homing, it is necessary for the birds sent to Galveston and out over the Gulf generally to be able to pass one or more nights upon the water, it occurred to us to make some tests as to the swimming ability of the birds, their ability to rest on the water over night, etc. We report the following notes made by the senior author in 1910:

TESTS UPON THE WATER HABITS AND INSTINCTS OF NODDY AND SOOTY TERNS.

1. One noddy and one sooty were carried into the water and gently placed on the surface of the water. They arose with ease and flew away, none the worse for the test.

2. One noddy and one sooty were held in the water and at times immersed in it for a period of 10 minutes. The birds were then placed gently on the surface. The noddy immediately raised itself and flew over to the land. The sooty was helpless under these conditions. While in the water it floundered about, getting the tips of its wings out, but could not rise. Since it was becoming more and more water-logged, I rescued it and tossed it into the air. It immediately righted itself, and, gathering strength, rose higher and higher and flew away. When in the water the bird made no attempt to swim, nor did it turn instinctively towards the shore.

3. Two noddies were captured and carried into the water and plunged below the surface to a depth of 2 feet and released. When they reached the surface, both rose easily and flew away.

4. A large wire cage, 6 by 8 by 7 feet, having a 2-foot door, was carried into the water to a distance of 100 yards from the shore and left there during the test to be described. The cage was entered through a wire door in the side. It was sunk to a depth of 2 feet. This left 5 feet between the surface of the water and the top of the wire door. (See plate 5.) Two noddies and two sooties were placed in this cage at $5^h\ 15^m$ p. m. They began flying up and attempting to get out. All were able to alight on the water and to rise from it, the noddies in a beautifully coördinated way. When they alighted they would fold their wings carefully and tightly against the body and hold the head and breast high above the water, swimming as smoothly and as beautifully as a swan, and showing perfect at-homeness in the water. (See plate 5, fig. 1.) The sooties, on the other hand, arose and alighted clumsily—head barely out of the water, wings not held up—sometimes, indeed, they were spread out. The sooties, however, could swim perfectly. For about 2 hours the birds attempted to fly out, the noddies making the more vigorous resistance. The birds then became relatively quiet. I left them undisturbed until $8^h\ 30^m$ the following morning.

The noddies were in first-class condition at that time and were not in the slightest degree bedraggled or water-logged. They swam as easily and as perfectly as in the afternoon of the day before. As I approached the cage, they arose and hovered for more than a minute in the air, then alighted. I opened the door and the birds swam out and rose immediately from the water, one going at once to its nest, the other out over the water. One sooty was dead; the other was just barely able to keep its head above water; the feathers were water-soaked and the bird was shivering. Plate 5, fig. 1, shows the condition of the birds at this time. The two noddies are in perfect condition. The one live sooty is shown in the left-hand corner of the photograph. I rescued it and put in on the warm sand, where it remained and slept without moving from its tracks for nearly 2 hours. A commotion in the colony then alarmed it and it arose and started out over the water. It flew to a distance of 100 yards and fell into the water. The other birds of the neighborhood immediately began to hover over it in the peculiar way shown in our earlier study. I swam out and rescued the bird, tossing it high in the air. It flew the remaining distance to the shore and again sunned itself for more than an hour.

5. Two noddies and two sooties were put into the cage as before, this time at 11 a. m., so that I might be able to watch their behavior and rescue the birds in case of disaster. The island was generally quiet and I watched them through a field-glass so that I might not

disturb them. The birds behaved as in the previous test. After a time they ceased to make attempts to escape and settled down in the water. In about 2 hours one sooty could no longer rise from the water, and in 4 hours the second sooty became entirely water-logged. At 6 o'clock, seeing that the sooties would soon be entirely helpless, I rescued them. At my approach the noddies flew up and hovered in the air as if they had not been through any difficult experiences, and when I opened the door they swam out and flew away. The sooties were quite helpless. I took them in and kept them over night in a warm place. On the following morning they were apparently as fresh as ever.

These tests seem to show that the noddy is as at home in the water as in the air. It is quite clear that it could stay on the open water in calm weather indefinitely, and that even in rough weather it could probably last out by alternately alighting on the water and then flying up. It would seem from the above tests that the sooty would be unable to last over night in the water unless some aid were at hand. Incidental observation leads me to suppose that the sooty is the better flier. As was brought out in an earlier paper, the sooty joins the man-o'-war birds and the pelicans in difficult flying feats. These birds gathered in crowds and circled around and around, going up completely out of sight. Again at night the sooty is far more on the wing than the noddy. Furthermore, the sooty flies higher, much higher, than the noddy. In coming in at night from the water the noddy nearly always takes the lower strata, flying usually not more than a few meters above the surface. The sooty, on the other hand, flies high. Occasionally one sees two sooties darting off together and keeping parallel courses, no matter how the direction of the flight may be varied. Both wheel to the right or to the left, down or up, in a way quite similar to the evolutions of the pigeon. One never sees noddies engaged in this form of play. It would seem that the two species differ markedly in their behavior towards the two elements, air and water.

6. I became anxious to find out if the fate of the sooty forced to spend the night upon the water were changed if small pieces of drift-wood were at hand. Two noddies and two sooties were put into the cage as before. This time I had tethered against the tide 4″ by 6″ by 6″ pieces so that they floated free and were held away from each other. The sooties alighted first on the water. The two noddies began alighting on the blocks almost immediately (plate 5, figs. 2 and 3). As I write, 10 minutes later, both sooties are in the water and both noddies on the blocks. The blocks keep turning over in the water, since the tide is running out rapidly and it is hard for the birds to keep their footing. The sooties make little or no effort to take the other two blocks. They sometimes fly up in the air, but rarely attempt to take the blocks. One just caught the block (plate 5, fig. 2), but flew up immediately and did not again attempt to alight on it. The noddy when alighting goes always to the block. At the end of the first ten minutes one sooty began constantly to climb up and ride on the block but could not stay on very well. The other sooty had not made the slightest attempt to alight on it. The noddies at the end of twenty minutes never alighted on the water. At the end of 25 minutes one sooty, which had been constantly in the water, was no longer able to rise from it (plate 5, fig. 4). Two hours later the bird which was unable to rise escaped by reason of the fact that the tide had dropped below the top of the door in the side of the cage, which I had neglected to close. The other three birds each possessed a block and were calmly standing upon it, making no efforts or struggles to escape. At $1^h\ 15^m$ the sooty had perfectly mastered the block and seemed quite content. It apparently had to learn to use the block and did not go instinctively to it. At 2 p. m. I captured another sooty and placed it in the cage. The behavior of this bird was wholly similar to the above. Its efforts disturbed the other sooty, which began to make struggles to escape, becoming uncoördinated in its movements, and finally dropping into the water. At $4^h\ 30^m$ both sooties were in such poor condition that I decided to release them. The noddies were still in as good condition as before.

7. On June 9, I tethered heavy pine boards 1″ by 6″ by 8″ as floats. They were fastened at both ends, so that no matter which way the tide was running they would remain relatively stable. Under these conditions the sooties passed the night comfortably and were in perfect condition on the following morning.

It seems quite clear from these experiments that if the sooty could find fairly heavy pieces of drift-wood in the Gulf or beds of sea-weed, which are probably always present, it would be able to pass an indefinite period of time upon the water. Up to the 1913 experiments I had supposed that this inability to pass

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METHOD OF TESTING SWIMMING BEHAVIOR.

Fig. 1. Noddies and sooties after spending night on the water. The two noddies are in good condition. One sooty is drowned and the other is shown, in lower left hand corner, in a very poor condition.

Fig. 2, 3. Illustrating the noddies' quick possession and retention of the blocks. The sooties have left the blocks and are slowly becoming water-logged.

Fig. 4. The test with floating blocks. One sooty failed to use the block and is in poor condition.

the night upon the water in the absence of drift-wood accounted for the failure of many of the birds to return to Bird Key. The 1913 experiments upon homing between Galveston and Bird Key (p. 54) seem to show that entire reliance can not be placed upon the results of the experiments we have just considered. In the Galveston tests many of the birds were out upon the water (apparently) during periods ranging from 5 nights to 17 nights. Hence we must conclude either that drift-wood was abundant or else that the sooty when not confined can spend the night upon the water. If our results are taken at their face value they would seem to force the conclusion that the Galveston sooties were able to find something to rest upon during the night.

RETENTION OF NESTING HABITS.

In controlling experiments upon homing it becomes essential to know (1) the length of time the bird left on the nest (O) will care for the nest, egg, young, etc., and (2) the length of time nest habits will be retained by the bird which has been sent away (R). The necessity for data on (1) arises by reason of the fact that as soon as the bird left at home begins to desert the nest, other birds steal the straws, if it is a noddy nest, or fill it with sand, disarrange the egg, etc., if it is a sooty nest. Nests so disturbed will not be taken by birds returning from the experiments on homing. Since the observer can not keep all of the nests under observation at any one time, it is essential that they be kept in good condition by the bird remaining at the nest. In order to control this factor perfectly it would be advisable in future experimentation to invert wire cages (with open doors) over the nests several days before capturing the birds which are to be sent from the island. These nests could be watched and as soon as the O at any given nest begins to desert, the door of the cage could be closed up until R was expected. R having been habituated to the cage before he left, would not hesitate to enter it on his return if the door were left open or to perch upon it if the door were closed.

The necessity for data on (2) appears when we consider the possibility of the effect of loss through disuse (or waning) of nest habits, upon the possibility of locating the bird (R) upon its return. If nest habits, habits formed to a particular nest, to mate, etc., are lost at the end of 2 or even 3 weeks, it is unlikely that the total number of birds returning from long trips can ever be observed. Fairly complete data have been gathered upon (1).

OBSERVATION UPON BEHAVIOR OF BIRD LEFT AT NEST (O) AFTER REMOVAL OF NEST MATE (R).

So many observations have been made upon this phase of the subject, that only a small number of our records will be given. We present in table 4 the behavior of 24 birds whose mates had been sent to Galveston. The mates were removed May 15, 1913. During the first 6 days the O's carefully guarded the nests, usually sitting closely upon the eggs. Occasionally short trips for food and water were made. This, in the case of the sooties, means a tremendous change in habits, since they ordinarily leave the nest for 24 hours at the nest shift.

Records similar to the above were kept upon several different occasions, but the results were identical. Summarizing (1), we find that both noddies and sooties, when left alone at the nest, remain on guard and brood the egg

closely for at least 6 days. There then ensues usually a period where, in most cases, the birds begin to guard the egg less closely, leaving it entirely for long stretches of time, and on returning merely stand or sit near it. Complete desertion finally occurs. Individuals differ greatly as regards time of desertion, as may be seen by examining table 5, condensed from table 4.

TABLE 4.—*Behavior of 24 birds whose mates had been sent to Galveston.*

A=absent. P=present. MR=Mate returned. Pa.=Present but not sitting on nest. D=deserted. CAM=courted by another bird.

	7 days.	9 days.	11 days.	14 days.	19 days.	22 days.	27 days.	Remarks.
Sooties.								
No. 1	A	A	A	MR	..	..	..	Both birds hung around nest but never completely resumed care of egg.
2	P	A	A	P	Pa.	D	D	Deserted completely.
3	P	A	A	A	D	D	D	Deserted completely.
4	P	A	P	A	P	D	D	Deserted completely.
5	P	P	P	P	MR	..	..	The pair completely resumed nesting activities.
6	A	A	A	P	P	Pa.	D	Very faithful to end of observation.
7	P	A	A	P	A	D	D	Deserted.
8	P	P	A	MR	..	..	..	Pair resumed nesting activities.
9	P	P	MR	..	..	..	..	Pair resumed nesting activities.
10	P	P	A	MR	..	..	..	Pair resumed nesting activities.
11	P	A	P	MR	..	..	..	Pair resumed nesting activities.
12	A	A	A	MR	..	..	..	Completely resumed nesting activities, although had apparently deserted.
Noddies.								
No. 13	A	P	P	P	Pa.	D	D	Deserted.
14	P	P	P	A	Pa.	MR	..	The pair completely resumed nesting activities.
15	P	A	P	P	A	D	D	Deserted.
16	P	Pa.	A	P	A	D	D	Deserted.
17	P	A	P	P	D	MR	..	Deserted, but R when he returned took another nest mate and resumed (incompletely) nesting activities.
18	P	P	CAM	MR	..	..	..	Nesting activities not resumed.
19	P	P	P	P	A	P	P	Faithful to end of observation.
20	P	P	MR	..	..	..	..	Pair resumed nesting activities.
21	P	P	Pa.	A	D	..	..	Deserted completely.
22	A	Pa.	MR	..	..	..	..	Pair completely resumed nesting activities.
23	Pa.	CAM	D	..	..	..	..	Deserted.
24	P	P	P	MR	..	..	..	Completely resumed nesting activities.

TABLE 5.

No. 2 deserted at end of 19 days.
3 deserted at end of 7 days.
4 deserted at end of 19 days.
6 did not desert during limits of observation (27 days).
7 deserted at end of 14 days.
13 deserted at end of 19 days.
15 deserted at end of 14 days.
16 deserted at end of 19 days.
19 did not desert during limits of observation (27 days).
21 deserted at end of 14 days.
23 deserted at end of 11 days.

Only birds whose mates did not return are listed in this table. The data in all of these studies on retention of the nest suffer by reason of the fact that sex was not determined. No. 23, deserting at the end of 11 days, was undoubtedly a female.

RETENTION OF NESTING HABITS AND NEST LOCALITY IN THE BIRD REMOVED FROM THE NEST (R).

In order to test the limits of retention of nest locality, 10 noddies and 10 sooties were captured and their nests plainly marked on May 12, 1913. They were individually marked with oil paints, carried to Loggerhead Key, and kept in a large fly-, mosquito-, and ant-proof cage.* We planned to release these birds at varying intervals and to note whether they returned and resumed normal nesting activities with their mates. The experiment was planned to give the limits of time within which complete resumption of nesting activities might take place after forcible detention on Loggerhead. The test was completed as follows: On the day any given bird was to be released on Loggerhead the experimenter stood before the nest while an assistant took the motorboat and went to Loggerhead to release the bird. The watches of the experimenter and the assistant were set together. The following summary is taken from the notes of one of the observers.

11 days:

Noddy No. 11 was released at 4^h 24^m p. m. after spending 11 days in captivity. Upon release at Loggerhead the bird flew out to water and thoroughly bathed. Thereupon it flew off straight to Bird Key. Bird was fed before leaving. Arrived at rim of nest at 4^h 34^m. Total time 10 minutes. Flew away an instant after alighting. No sign between mates was made. O had been very faithful. Upon R's return did not attack, as it surely would have done had R been a stranger. After 26 minutes R returned to island and flew to house top. Remained there 8 minutes, then came down and arriving at nest bowed (plate 3, fig. 1) to mate. Mate bowed in return. R did not sit on egg that day, but brooded it the following day in a normal manner.

15 days:

Noddy No. 17 was released at 11^h 33^m a. m. after 15 days. Arrived at nest at 11^h 57^m. Time, 24 minutes. Left nest and did not reappear before observer left at 1^h 15^m. O was not on the egg but on nearby limb. Bird did not appear again that day while I was present. On the following morning I saw O repeatedly drive R away. Friendly relations were never resumed.

Sooty No. 12 was released at the same time. This bird never appeared at its nest.

16 days:

Noddy No. 18. Released at 5 p. m. after 16 days. Would not leave Loggerhead. Flew to water, thereupon to bow of one of the boats, then underneath dock. Finally disappeared from Loggerhead but never appeared at its nest.

Sooty No. 5. Released at same time as No. 18. Flew out over gulf to Southwest, then turned and started towards Bird Key. Never was observed at nest.

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*Very fine galvanized wire netting inclosed all sides of the cage. The cage was floored and covered with wooden boards. It was mounted upon four legs, 3 feet in height, and the bottoms of the legs were inserted into large pails kept filled with water. Fresh sand was kept on the floor of the cage. The birds were fed with minnows. At first they would not eat and some had to be forced to eat. After 4 or 5 days the majority of them began to eat. For some reason many of the birds lost flesh rapidly and died, even in cases where they apparently ate the normal number of minnows. Of the 20 birds in captivity only 13 lived through the experiment.

18 days:

Noddy No. 16. Released at 9^h 30^m a. m., 18 days in captivity. Refused at first to leave dock. When driven away started north after bathing. Was not observed at nest until 6 days later. He was then sitting on the rim (at 5 a. m.). Mate was near. Nesting activities not resumed. Probably on Bird Key on night of day released, but left for feeding-grounds every morning before observations could be made.

Noddy No. 19. Released same hour as No. 16. On release joined No. 16 in flight. Seen near nest on following morning. Nest not resumed. Mate had completely deserted.

20 days:

Sooty No. 7. Released 8^h 50^m a. m. after 20 days. Bird very weak. Would not leave Loggerhead and finally died there.

Sooty No. 8. Released as above. Flew off and headed north. Never appeared at nest.

22 days:

Noddy No. 20. Released 3^h 31^m p. m. after 22 days. Flew north. O had taken another mate. R was not observed until 3 days later (probably was driven off by O). When observed R was not sitting egg. Nest was finally abandoned.

25 days:

Noddy No. 13. Released 8 a. m. after 25 days. Not observed at nest until 3 days later. Made no effort to sit egg. O not near nest.

Noddy No. 14. Released with No. 13. Spent 5 minutes in water, then flew west. In good condition. Not observed until 3 days later. Mate not near. Made no effort to sit egg.

26 days:

Noddy No. 15. Released 11 a. m. after 26 days. Spent 5 minutes in water, then flew west, appearing at nest at 12^h 30^m p. m. Was not sitting on egg. Two days later was observed sitting on limb above mate. Mate was on the egg. Later observations showed that O had accepted another mate. R often appeared and hung around, but never forced out the intruder.

A summary of these field notes is given in table 6:

TABLE 6.

No. 11 at end of 11 days returned to nest.	No. 13 at end of 25 days returned to nest.
17 at end of 15 days returned to nest.	14 at end of 25 days returned to nest.
16 at end of 18 days returned to nest.	15 at end of 26 days returned to nest.
19 at end of 18 days returned to nest.	18 did not return to nest after absence
20 at end of 22 days returned to nest.	of 16 days.

None of the 4 sooties living through the experiment returned to the nest.
Except No. 11, the relations with mate were not resumed.

From this series of notes we see either that the conditions were not favorable to the sooties or else that locality and nesting habits are not retained by them for as long a time as by the noddies. From data offered below it would seem that the unnatural conditions offered by captivity at Loggerhead produced a waning of the nesting impulses, if not loss of retention of nest locality, and furthermore, that habits of responding to a given mate are lost before the habits of responding to a given nest (place). The system of observation at the nest, however, had not been perfected. When hundreds of nests were under observation by one investigator, it might easily have been possible for him to have timed his observations in such an unfortunate way as to have missed the appearance of the Rs at the nests. A reference to page 42 shows that the sooty when not brooding the egg disappears for 24 hours and sometimes 48. Furthermore, these birds usually leave their nests at daybreak, before there is sufficient light for identification.

Data supplementing the above may be obtained by referring to table 4, p. 42, which gives a record of 13 birds returning from the Galveston trip (places are marked by the words "mate returned"). The length of time the birds were out is given in table 7.

TABLE 7.

Sooty No.	1 returned after 14 days.	Noddy No.	14 returned after 22 days.
	5 returned after 19		17 returned after 22
	8 returned after 14		18 returned after 14
	9 returned after 11		20 returned after 11
	10 returned after 14		22 returned after 11
	11 returned after 14		24 returned after 14
	12 returned after 14		

In this test, where the birds were not held in captivity, we find 7 sooties returning to the nest after intervals varying from 11 to 19 days and 6 noddies after intervals varying from 11 to 22 days. In most cases where the nest had been maintained by Os, the Rs on *returning resumed normal nesting activities with their former mates.* We thus see that under these more natural conditions the nesting impulse remains strong from 2 to 3 weeks and that the habits of reacting to a particular nest and nest mate are also retained during this period.

The question arises, of course, as to how many birds returned to Bird Key after the various expeditions but escaped observation through the *waning of the nesting instincts or the loss of nest habits.* This question is not wholly answered by our experiment. It seems improbable that birds returning under 2 weeks could have escaped observation. If return was delayed much longer than this (as was perhaps the case in many instances) it is probable that the presence of the birds at the nest was too sporadic to be detected by non-continuous observation.

GENERAL TECHNIQUE OF THE HOMING EXPERIMENTS.

The following routine was adopted on Bird Key in preparing for an experiment on homing. Roads were first cut through the thickly populated noddy and sooty districts. These roads may be cut at any time before May 15 (they should not be cut later than this because of the number of eggs which are on the ground). It is easy to capture both noddies and sooties on these roads, and easy to observe their return. Where, in the course of a season, so many birds are likely to be on the road, it makes it very much easier to take all of the Galveston birds, *e. g.*, from one road, the Key West birds from another, etc. As one passes down the roads the boldest of the birds will stay on the nests, or if they do leave momentarily, they will fly back while the experimenter is standing close to the nest. These bold birds are the ones always captured. Before passing down the road for the purpose of capture, stakes about 12 inches long and 1 inch square are made. A large Dennison tag and a small tag are attached to the end of the stake, the small tag being attached loosely. The two tags bear identical legends. The large tag will have written upon it in water-proof ink, *e. g.*, "Sooty, Galveston, removed May 16, marked with scarlet lake, 3 bars on head and neck." When the bird is captured the stake is pushed down into the sand if a sooty or tied to a convenient twig if a noddy very near to the nest (plate 6, figs. 1 and 2). The small tag, bearing a duplicate of the above legend, is pulled off. The bird and small tag are handed to an assistant, who ties the tag around the bird's neck and puts the animal in

a portable cage. When enough birds have been collected the lot is taken back to the house and marked with oil paints as indicated by the cards attached to the birds' necks. Plate 6, figs. 1 and 2, show also the clearness with which the markings appear after the return of the bird. Not until 1913 did we realize the importance of having individual compartments of large size in the carrying cage, and although we used individual compartments in 1913 they were not large enough, so that it may safely be said that none of the experiments to be reported has been carried out under ideal conditions. Certainly if any other work is ever to be attempted at Bird Key it is absolutely essential to have a large compartment for each bird, and a supply of minnows sufficient to last the whole trip. The most convenient way to perfect the technique of feeding the birds on the trip is to send a refrigerator with the birds and to keep the minnows stored in this.

PREVIOUS EXPERIMENTS ON HOMING AT TORTUGAS.

In order that the present paper may give a complete record of the homing work done at Tortugas, we copy the following summary from the 1907 publication.

Experiment I.

Six noddies were captured one evening and marked characteristically and individually with oil paints. These birds were put on board the laboratory launch, which happened to be making a trip to Key West on the following morning. The nests of these birds were all close together and were tagged with a large card in order to facilitate observation. Two of the birds were released at Rebecca Shoals Light, 31.38 km. (19.5 statute miles) from Bird Key; two at Marquesas, 72.75 km. (44.75 statute miles); and two at Key West, 106.02 km. (65.8 statute miles). I kept their nests under constant observation the whole day long. Naturally, since the birds had been without food for some time, and since I had no guarantee that they would immediately seek the nest after reaching the island, I expected the return to the nest to be irregular. The results were as follows:

The two Rebecca birds, released at $9^h\ 30^m$ a. m., returned about 12 noon.

The two Marquesas birds, released at $2^h\ 15^m$ p. m., returned together at 4 p. m.

The two Key West birds were released at $6^h\ 30^m$ p. m. One returned at $7^h\ 30^m$ a. m. the next day, the other at $5^h\ 05^m$ p. m. These two birds, bearing out my statement that these terns do not fly at night, probably slept in the neighborhood of Key West and left early the next morning. A heavy gale and rainstorm set in very shortly after these birds were released and I doubted very seriously whether they would ever return. Apparently one of the birds was not affected by the storm, while the other was probably blown from its course. The respective mates of these birds remained on the eggs the entire time, going neither for food nor water (?).

Experiment II.

Three noddies and two sooties (one of the sooties was known to be a male) were captured and marked as above. Their nests were likewise prominently marked. On the early morning of Thursday, June 13, these birds were put into a large insect cage and given in charge of Dr. H. E. Jordan, who was returning to New York. He carried these birds *via* the government tug to Key West. There food was purchased for them (minnows). At 3 a. m. Friday the 14th, Dr. Jordan boarded the Mallory boat *Denver*, which left at that time for New York. On board the boat the birds were both watered and fed. On Sunday, the 16th, at $9^h\ 20^m$ a. m., the birds were released at lat. 35° 8′, long. 75° 10′ (12 miles east of Cape Hatteras, approximately). The wind was fair for several days after the birds were released. I kept their nests under constant observation, but had almost given up hope of their returning when, to my surprise, on June 21, at $8^h\ 30^m$ a. m., I found both marked sooties on their respective nests.

None of the marked noddies was ever found on its old nest, but several days after the sooties had been observed at their nests, by chance I observed one of my marked noddies

attempting to alight on its nest. On account of the mate having formed new "affiliations" this was not permitted, and I immediately lost track of the bird. I have little doubt that the other noddies also returned to the island, but likewise were not permitted to return to their nests.

The distance from Hatteras to Bird Key in a straight line is approximately 1,367.9 km. (850 statute miles). The alongshore route, which is the one in all probability chosen by the birds on their return, since they were gone several *nights*, is approximately 1,739.6 km. (1,081 statute miles).

EXPERIMENT III.

On Monday, July 8, two noddies and two sooties were captured and marked and given into the charge of Dr. Robert Hartmeyer, who was returning to Germany by way of Havana. The birds were in such poor condition, owing to the enormous strain of several days' feeding of their then quite large young, that we decided to release them at Havana instead of taking them farther out. On the 9th the birds were carried by Dr. Hartmeyer on board the government tug and taken to Key West, where they spent the night and part of the following day, the 10th. They were carried in Dr. Hartmeyer's stateroom to Havana on the night of the 10th. Early in the morning of the 11th the birds were released in Havana Harbor. All returned to Bird Key on the 12th. Since they had had to spend 3 days without food or water, they were in poor physical condition. They probably spent one day and night around the shores of Cuba, leaving there early in the morning of the following day. The noddies were observed on their nests at 7 a. m. while the sooties were noted for the first time at $6^h\ 30^m$ p. m. of the same day.

The distance in a straight line from Havana to Bird Key is approximately 173.8 km. (108 statute miles).

RECORD OF EXPERIMENTAL WORK ON HOMING IN 1910 AND 1913.

THE FLIGHT FROM KEY WEST.

On May 16, 1910, 12 noddies, 12 sooties, and 4 man-o'-war birds were captured and marked individually as described below. We had intended to ship these birds to Key West on the 17th in order to send them to Galveston on the

TABLE 8.—*Flight of noddies from Key West.*

Marking.	Returned.	Days out.
Yellow, irregular splotches on right wing	May 20, noon	1 d. 21½ h.
Yellow band encircling neck	May 20, $10^h\ 30^m$ a. m.	1 d. 20 h.
Yellow line between wings	May 21, 6 a. m.	2 d. 15 h.
Yellow, irregular splotches, right wing	May 19,[1] 10 a. m.	19½ h.
Yellow, solid on right wing	May 19, 5 p. m.	1 d. 2½ h.
Yellow, two vertical lines on head	May 19,[1] 8 a. m.	17½ h.
Yellow line from apex of head down to tail feathers	May 19,[1] 8 a. m.	17½ h.
Yellow, splotched head and neck	May 19,[1] 8 a. m.	17½ h.
Yellow, two heavy vertical bars on head and neck	May 19,[1] 10 a. m.	19½ h.
Yellow, solid on head	May 20, $10^h\ 30^m$ a. m.	1 d. 20 h.
Yellow, solid on left wing	May 20, 8 a. m.	1 d. 17½ h.
Yellow, horizontal lines on head	May 19,[1] 8 a. m.	17½ h.

[1] These birds probably returned afternoon of day before, *i. e.*, same day released. All the birds had a strong northeast wind with them.

18th by Dr. J. F. McClendon.* On the 17th a heavy storm set in and the naval tug could not leave Tortugas. On the 18th, since there was still a chance to catch the Galveston boat, they were carried by Dr. McClendon to Key West. The tug arrived too late to connect with the Galveston boat and the birds were accordingly released May 18 at $2^h\ 30^m$ p. m. in Key West Harbor, 65.4 knots

*I am very grateful to Dr. McClendon for his help in this experiment.

due east of Bird Key. Thinking that Dr. McClendon had made the Galveston boat, we did not visit the nests that afternoon. On the following morning, in making the rounds of the nests, we observed several of the marked birds on their nests. We began then to make systematic observations and report here the first time each bird was observed at its nest. In all cases these are the actual first observances of *appearance on* the nest, but in view of what has been said earlier (p. 45) it is possible that the birds may have attempted on earlier occasions to take possession of the nest. Tables 8 and 9 show the time of release, the markings of the bird, and the apparent time of return.

On June 10, at 2^h 30^m p. m., 1 man-o'-war bird was observed. That the others were not observed is not remarkable, since:

(1) There was no adequate stimulus to cause them to return. The man-o'-war bird is at home on any coast or island south of Hatteras. He merely acts as a scavenger wherever he may find himself. The results might be quite different if we were to carry out similar experiments upon him in the Bahamas at the time of the nesting season or were to rear these birds as pets.

TABLE 9.—*Flight of sooties from Key West.*

Marking.	Returned.	Days out.
Scarlet lake, two circles on breast	May 24, 6 a. m.	5 d. 18½ h.
Scarlet lake, three horizontal dots on breast	May 25, 6 a. m.	6 d. 18½ h.
Scarlet lake, three diagonal dots on breast	May 19, 8 a. m.	17½ h.
Scarlet lake, three horizontal bars on neck and breast	May 20, 8 a. m.	1 d. 20½ h.
Scarlet lake, solid red breast	May 19, 8 a. m.	17½ h.
Scarlet lake, three vertical dots on breast	May 19, 8 a. m.	17½ h.
Scarlet lake, irregular lines on breast	May 20, 8 a. m.	1 d. 20½ h.
Scarlet lake, diagonal lines, left to right on breast	May 27, 6 a. m.	8 d. 18½ h.
Scarlet lake, large splotches on breast (male)	May 28, 6 a. m.	9 d. 18½ h.
Scarlet lake, diagonal line, right to left on breast	May 30, 3 p. m.	11 d.
Scarlet lake, three horizontal bars on breast	.....	.....
Scarlet lake, three vertical lines on breast	.....	.....

(2) The second difficulty lies in the fact that many of the birds are deeply hidden by the bushes. For some reason not yet found out they line the edges of the bushes wherever the terns are nesting; we now suspect for the purpose of collecting the eggs and young. They often roost and rest quite low and it is impossible to cover the whole island adequately with the glasses. Even if the other birds did return, the chances are much against the fact being discovered.

Before the above 4 birds were released, 2 man-o'-war birds were released by Dr. Mayer in Key West, on May 6, at 2^h 13^m p. m. One of these birds returned on May 11. We happened to get close enough to observe him without the aid of glasses. On account of the difficulty of noting their return, future work with them was discontinued.

The records that the terns made in this Key West flight are valuable in view of the fact that they show that the birds, when covering *short distances, require,* in *many cases, as much time as birds covering longer distances.* In the above tables the time ranges from 17.5 hours to 11 days. Many of the returns from distances greater than 500 miles did not require more than 3 to 5 days (pp. 50 and 58).

As this Key West flight gave us a set of "trained" birds, we tested the speed at which the noddy and sooty can fly, as follows: 3 noddies and 1 sooty, which

had already flown from Key West on the above flight, were again captured and sent in a hooded cage to Key West; they were released in Key West harbor at 2 p. m. (See table 10.)

TABLE 10.

Birds.	Time.	Remarks.
Noddy 1 Noddy 2 Noddy 3 Sooty 1[1]	5^h 45^m 5^h 45^m 5^h 45^m 5^h 45^m	All birds returned together. They came in with a great flock of other birds which had been feeding on the open water (the return of the birds in large flocks is usual just at nightfall). The birds probably made good time until the feeding-ground was reached, and then leisurely ate. It would be a far better test to take well-fed birds and release them about 5 p. m. Owing to an oversight this test was not made in 1913.

[1] The young bird was present in the nest of this sooty.

Since Bird Key is only 65.4 miles from Key West, the time (3^h 45^m) is not very rapid when compared with that in which the homing pigeon could cover a short distance like this (at the rate of 1,200 to 1,600 yards per minute).

ATTEMPTED FLIGHT FROM NEW YORK AND EN ROUTE.

On the afternoon of May 19, 1910, birds for a New York–Tortugas flight were captured. They were taken to Key West, leaving the island at 8 a. m., May 20, on the *Vellela*. It was stormy and the *Vellela* got into Key West too late to catch minnows for the trip. While we felt sure that this doomed the expedition to failure, nevertheless we purchased a large fish, which we cut up into strips, substituting it as a food supply in place of the minnows. The Mallory boat *Concha* left Key West harbor the night of May 20 at 10^h 30^m, the birds being in charge of Mr. Wilson. The cage was hooded and in addition was carried in the hold of the vessel. The first lot of birds was marked as shown in table 11.

TABLE 11.—*Night release.*

Sooty No. 1. Yellow, line down back, 1 dot on breast.
Sooty No. 2. Yellow, both wings splotched.
Sooty No. 3. Yellow, right wing splotched.
Sooty No. 4. Yellow, 3 vertical bars on left wing.

These 4 birds were released at 7^h 30^m p. m., 365 miles from Bird Key, *at night* (immediately after twilight had disappeared). Only 1 bird from this lot returned, requiring approximately 4 days. On release, 1 of the 4 birds started due south and the others southeast. The weather was fair until the 24th. On the evening of the 24th a heavy southeast wind set in, which made it impossible for the birds to make Bird Key. The rest of the birds were carried to New York. They were marked as shown in table 12.

TABLE 12.—*Released in New York harbor.*

Sooty:	Noddy:
No. 1. Yellow, solid breast.	No. 1. Red, solid head.
2. Yellow, one vertical bar on breast.	2. Red, vertical bar on head.
3. Yellow, one large horizontal bar on breast.	3. Red, one horizontal bar on head.
4. Yellow, two large horizontal bars on breast.	4. Red, right wing and head solid.
5. Yellow, vertical bars on breast.	5. Red, left wing and head solid (died in passage.)
6. Yellow, splotched breast.	6. Red, splotched head.

On account of the food conditions the birds became weaker and weaker. They were released in New York harbor on Tuesday morning, May 24, at 4^h 30^m, in a fog. On release they left the ship and flew to a distance of 200

yards, alighting in the water. One sooty was too weak to maintain itself in the air. These birds probably would have had to fly 1,000 miles before they could find a suitable fishing-ground. As has been mentioned, they eat only live minnows, which hop over the surface of the water. In view of this fact it is not surprising that none of the birds returned to Bird Key. Furthermore, the spring in 1910 was much delayed. The air and water in New York harbor were unusually cold, but the other weather conditions at first were favorable. There were south and southeast winds on the 24th and 25th, while a heavy northeast wind with squalls began on the 26th and continued through the 27th. From May 28 to June 1 a flat calm prevailed. On the 1st of June a heavy south wind set in.

THE FIRST GALVESTON TRIP, 1910.

The birds as marked below were captured and put into a hooded cage on the afternoon of May 29, 1910. They were carried to Key West on the *Physalia* May 30, and put on board the Mallory boat *Concha*, leaving Key West in the afternoon of June 1, in charge of Mr. Wilson. Two releases were made. The birds in the first release were marked as set out in table 13.

TABLE 13.—*First release.*

Noddy:	Sooty:
No. 1. Yellow, one horizontal bar on head.	No. 1. Red, one horizontal bar on breast.
2. Yellow, two horizontal bars on head.	2. Red, two horizontal bars on breast.
3* Yellow, one vertical bar on head.	3. Red, one vertical bar on breast.
4. Yellow, two vertical bars on head.	4. Red, two vertical bars on breast.

These birds were released June 3 at 4^h 45^m a. m., *en route* for Galveston, in the open ocean. The *Concha* was then 470 knots from Key West. Since Bird Key is 60 knots from Key West that distance must be subtracted, leaving a distance from Bird Key of 410 knots, or approximately 461 statute miles. On release all birds with one exception started east. That one headed west and continued for about 200 yards, then turned suddenly and started east. The birds all flew up about 20 to 30 feet, then downward, and started off within a few feet of the water.

The returns were as follows:

Noddy No. 1 returned night of June 6; time, 3 days.
Noddy No. 4 returned night of June 6; time, 3 days.
Since Noddy No. 3 died we have 2 noddies out of 3 returning from this trip.
None of the sooties was noted with certainty at Bird Key. This is the first successful flight over open water where no shore-line is visible that has ever been recorded in any homing bird. The homing pigeons' flight over open water, even where there is a coast-line, rarely exceeds 200 km.

What makes this flight all the more surprising is the fact that the birds had a strong head wind against them all during the first day. The second day was calm, while on the last day heavy winds were again against them. Thirteen other birds, marked as shown in table 14, were taken to Galveston harbor.

TABLE 14.—*Galveston harbor release.*

Noddy:	Sooty:
No. 1.* Yellow, solid head.	No. 1. Red, solid breast.
2. Yellow, splotched head.	2. Red, one bar left to right on breast.
3. Yellow, left wing solid.	3. Red, splotched breast.
4. Yellow, right wing solid.	4. Red, left half of breast.
5.† Yellow, both wings solid.	5. Red, right half of breast.
6.* Yellow, head and right wing solid.	
7.† Yellow, head and left wing solid.	
8. Yellow, line from head to tail.	

*Died in passage. †Nos. 5 and 7 were mates.

The 11 birds which survived were released in Galveston harbor Saturday morning, June 4, at 5 o'clock. These birds were in a very weakened condition, owing to the fact that we had not then begun to construct our cages with individual compartments. All the birds were carried in one cage. Mr. Wilson reported that in transit the birds piled up on one another sometimes 3 and 4 deep. As their claws are extremely sharp, this resulted in much injury to the birds underneath; their feathers were torn out and the skin made raw. Furthermore, they were not eating well, for we again had not deemed it necessary to provide minnows for the full trip. When they were released all the birds flew ashore and alighted upon the sand. They were released in a light head wind. On the following day a heavy wind set in against the birds and continued all day. On June 8 Mr. Wilson left Galveston for Key West. On June 9, at 4 p. m., he saw one of the marked sooties resting upon a piece of driftwood. The red marking was plainly visible to the naked eye and still more clearly to be seen with the glasses. The bird was seen by several other people, including Mr. Greenwood of Galveston and Mr. Brown of the First National Bank of New York. The distance out from Galveston was approximately 409 statute miles. Mr. Wilson is one of the zoological collectors of the Laboratory and is thoroughly familiar with both species. He reports that with the above exception neither of the two species was seen either going to or coming from Galveston farther out than 18 miles from Bird Key (boats leaving Key West bound for Galveston pass within 4 miles of Bird Key). This attempted return of the sooty is quite remarkable in view of the fact that the direction which he had chosen was almost due east (the Mallory boats steam due east and west on these trips for a large part of the way).

Even up to June 11 we had hopes that some of these birds would return, but on that day a heavy and continuous wind set in from the south against them, which finally ended in a storm against which no bird on the open water could contend. As the Galveston birds which we did get to return on our second attempt required about 6 to 11 days (see p. 58), it is probable that the birds on this first release were overtaken by the storm before they reached Bird Key.

THE FIRST MOBILE TRIP.

On Friday, June 3, 1910, we captured 7 noddies and 7 sooties, which were sent to Key West on the *Vellela* at daybreak June 4. They were marked as shown in table 15.

TABLE 15.

Sooty:
No. 1. Orange, cross on breast.
2. Orange, one vertical line on breast.
3. Orange, one horizontal bar on breast.
4. Orange, splotched breast.
5. Orange, solid breast.
6. Orange, left half neck and breast.
7. Orange, right half neck and breast.

Noddy:
No. 1. Bright greenish yellow, cross on head.
2. Bright greenish yellow, one vertical line on head.
3. Bright greenish yellow, one horizontal bar on head.
4. Bright greenish yellow, splotched head.
5. Bright greenish yellow, solid head.
6. Bright greenish yellow, left half head and left wing.
7. Bright greenish yellow, right half head and right wing.

It was found impossible to purchase a supply of minnows and more than half the birds died before reaching Mobile. Those which reached Mobile alive were released on Thursday, June 9. The weather conditions were favorable on the 9th and 10th. On the 11th a heavy wind set in which by night had veered around to the south, becoming a veritable wind-storm. On the 12th the storm from the south still continued. All day it was cloudy and windy. After this time we gave up hopes of the return of the birds. None appeared during the time of our stay at Bird Key.

THE SECOND MOBILE TRIP.

The birds were captured May 5, 1913. They were sent to Key West by the government tug *Peoria* at 8 p. m., May 6. On May 7, they were shipped from Key West by the Mallory Steamship *Alamo*, in charge of Dr. K. S. Lashley. They were marked with green as shown in Table 16.

TABLE 16.

No. 1. Right wing.	No. 7. Breast and left wing.
2. Left wing.	8. Breast and top of head.
3. Both wings.	9. Breast and both wings.
4. Top of head.	10. Two horizontal lines across breast.
5. Breast.	11. Cross on breast.
6. Breast and right wing.	12. Two vertical lines on breast.

As the care of the birds on board the boat is probably the most essential part of a successful experiment in homing, and as it has taken us some time to learn the proper methods of handling the birds, we give Mr. Lashley's report in full. It seems worth mentioning that it was not until this trip that we were able to get all of the birds to their destination in fair condition.

NOTES OF MR. LASHLEY ON THE CARE OF THE BIRDS DURING THE MOBILE TRIP.

May 6: Arrived at Key West with birds in good condition. Wing and tail feathers a bit broken. Purchased a large fish and cut it up to feed the birds, since I was unable to get any large supply of minnows. Gave the birds salt water, with which they wet themselves badly. Placed two pieces of cut fish in water in each compartment for the birds to eat. I do not wish to use forced feeding unless necessary. If they have not taken the fish by morning I shall offer them minnows which I have obtained, using forced feeding wherever necessary.

May 7, 10 a. m.: Each bird was offered minnows. Nos. 2 and 7 each ate 2. The others had to be forcibly fed. Birds are damp and badly frayed. The strips of large fish were not eaten by any of the birds.

May 7, afternoon: More minnows were offered; 2 were forced on each bird which did not eat of its own accord. Food records were kept separately.

Left Key West May 7, 11 p. m.: Birds were loaded on top of deck house. Cage was closely hooded with canvas.

May 8, 6 a. m.: Gave birds 6 minnows each. All but Nos. 3 and 9 ate without being forced. These two ate 2 minnows each and then refused. I forced 4 more on each at 10 o'clock and left 6 to 8 minnows in each compartment. Birds are keeping themselves clean.

May 8, 4 p. m.: Birds fed again. Nos. 3 and 9 again refused and had to be forced. No. 3 is more eager to escape than the others.

May 10: Tampa was reached. Made 2 trips to Ballast Point in order to obtain minnows. Obtained only enough for one feeding. Birds ate them eagerly. On account of accident *Alamo* tied up until afternoon of May 11 for repairs.

May 12: Only a few minnows left and these not keeping very well. Obtained a large fish from the steward, but birds would not eat it; then gave them the rest of the minnows, about 15 each, exhausting the supply. This was at 11 o'clock. There was no food for them in the afternoon. For the last 3 days I have put no water in the cages. The birds come half way out of the door to eat and so keep dry. They do not show the need of water and probably get enough with the fish to satisfy them. Nos. 3 and 9 are recovering and seem to be in better condition than they were on Friday.

May 13: We reached the outer harbor of Mobile late last night and anchored. Started in at 4 a. m. to-day. Waited until we were near land before releasing the birds. At 5^h 45^m we passed a long island of dredged sand about 5 miles out from Mobile. I released the birds at this point. The morning was bright and warm. Sun about 20° above the horizon. Light wind from east. Ship steaming due north. Land to eastward just visible. Sand bar 500 yards to west with land a couple of miles beyond.

No. 1 released 5^h 45^m: Feathers in good condition but damp; was frightened and flew off unprepared; alighted in water 100 yards from shore; arose instantly and flew on towards land; was attacked by laughing gulls; arose and flew away in a direction a little south of east, alighting and arising again frequently.

No. 2 released at 5^h 55^m a. m.: Conditions the same. By this time No. 1 had disappeared. Released No. 2 from port side with head towards land. Bird flew down from top of deck house towards water, then swerved in a direction a little south of east and flew rapidly and smoothly about 6 feet above the water.

No. 3 released immediately after No. 2: Conditions the same. It overtook No. 2 and both disappeared, still flying and holding their eastern course.

No. 4: Ran about on deck for a short time, stretching wings; then took flight to starboard and took the same course as Nos. 2 and 3.

No. 5: Drier and in better condition than the preceding birds. Ran about on deck for a minute. Took flight to westward, but immediately swerved around to east.

No. 6: Not in best condition. No. 5 did not fly until No. 6 was released and No. 6 attempted to follow him immediately without preliminary wing stretching. It struck the rail and fell to the deck, but recovered itself quickly and followed No. 5.

No. 7: On release stood on the roof of the deck house for 5 minutes, until after No. 9 took flight. Was dry and in good condition. Took flight from starboard to eastward with No. 10.

No. 8: Good condition. Took flight from port side, swerving around to east.

No. 9: In very poor condition. Damp and weak. Started to the westward, then flew to land. Made several false starts to eastward, returning to land. Was perched on a rock when I last saw it.

No. 10: In fair condition, but damp and frayed. Started west, but swerved around to east.

No. 11: In the same condition as preceding bird. Went off with it and was joined by No. 7.

No. 12: Dry and in good condition; turned eastward immediately.

None of the birds rose above the top of the deck house before taking flight. The direction finally taken by all was the same, although they were released in three groups which were, I believe, not visible to one another. No. 1 was the only one which went into the water, and this was due to some extent to the attack of the gulls. None of the others was attacked. After the preliminary movements they all flew low, not over 3 to 6 feet above the water. All were flying strongly when last seen.

Table 17 shows the number of minnows eaten during the Mobile trip.

TABLE 17.—*Number of minnows eaten on trip to Mobile.*

No. of bird.	Wed.	Thur.	Fri.		Sat.		Sun.		Mon.	Total.
			a. m.	p. m.	a. m.	p. m.	a. m.	p. m.	M.	
1	*3	*4	10	9	10	5	7	5	15	68
2	3	3	10	8	8	4	5	5	15	61
3	3	3	10	*9	0	*5	3	5	15	53
4	3	3	10	5	9	4	7	5	15	61
5	3	3	10	7	10	4	7	5	15	64
6	4	3	10	9	10	4	7	5	15	67
7	8	2	12	10	8	7	7	5	15	74
8	3	2	7	11	8	4	7	5	15	62
9	3	3	5	*3	0	*5	5	5	*15	44
10	3	3	10	3	11	4	7	5	15	61
11	10	3	10	10	10	4	8	5	15	75
12	4	3	7	9	10	4	9	5	15	66

*Forcibly fed.

BIRD KEY NOTES ON THE RETURNS FROM THE SECOND MOBILE TRIP.

On May 20 bird No. 2 returned, probably in the night of May 19, since it was observed at 7^h 30^m a. m. The marking was not erased in any way. The mate had deserted the nest. The marked bird was found sitting on a limb within 3 feet of the nest and was noticed near it for 3 days, usually sitting on a nearby limb. The egg and nest were not taken until the afternoon of May 23; then we saw the marked bird (a male) for the first time on the nest. The time required for return was about 7 days. This is the only bird which we ever observed returning from the trip.

The location of the nests was unfortunate. They were nearly all flat on the ground, the nesting material being placed in the grass. The grass grew rapidly and in a few days it was difficult to locate the nests. Furthermore, the rats took the eggs the moment the O's began to go for food. We replaced the eggs many, many times from nearby nests, but the rats robbed them so consistently that the O's, after a very few days, all deserted. This choice of the noddy nests was unfortunate from another side as well. Even when the nest is deserted or destroyed the occupants of the nest will often sit on limbs near the nesting place. On account of the absence of bushes near the nests of the Mobile birds, it would have been difficult to have located the birds had they returned. It must be said that on account of these difficulties we redoubled our efforts to locate the birds, sometimes sitting near the nests for 3 or 4 hours without leaving.

THE SECOND GALVESTON FLIGHT.

Twelve noddies and 12 sooties were captured on Bird Key at 2 p. m., May 15, 1913, and were left there that night. The numbers and markings of the birds are given in table 18.

TABLE 18.

Sooty:	Noddy:
No. 1. Red, dab on throat.	No. 13. Red, dab on throat.
2. Red, dab on head.	14. Red, dab on head.
3. Red, dab on right wing.	15. Red, dab on right wing.
4. Red, dab on left wing.	16. Red, dab on left wing.
5. Red, dab on breast.	17. Red, dab on breast.
6. Red, dab on both wings.	18. Red, dab on both wings.
7. Red, two dabs on neck.	19. Red, two dabs on back of neck.
8. Red, two dabs on breast.	20. Red, two dabs on breast.
9. Red, throat and right wing.	21. Red, throat and right wing.
10. Red, throat and left wing.	22. Red, throat and left wing.
11. Red, head and left wing.	23. Red, head and left wing.
12. Red, head and right wing.	24. Red, head and right wing.

On May 16 they were taken to Key West on the *Anton Dohrn*. On May 17 the birds were put on board the Mallory freight steamer *San Marcos*. Mr. Lashley's record of the care of the birds on the trip and their behavior during release follows:

The first food was given on May 17; 3 minnows each were given by forced feeding. From this time on the feeding was continued as shown in table 19. It will be seen that on the afternoon of the 19th no food was taken. Food was placed in the cages to see if the

birds would eat of their own accord; it was not eaten. On the whole trip only 3 birds learned to eat of their own accord. The others were fed entirely by forcing. We give the table below in order that the future investigator may know (1) that the birds may reach their destination in good condition when forced feeding has to be resorted to, and (2) because it will enable the investigator to know the number of minnows, etc., required to maintain metabolism.

TABLE 19.—*Food record on the Galveston trip.*

Birds.	May 18.		May 19.		May 20.		May 21.		May 22.		May 23.		May 24.		Total minnows consumed.
	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	
Sooties.															
No. 1	3	0	0	*	4	..	3	5	2	3	5	10	5	..	35
2	3	4	3	*	6	..	6	13	9	10	10	13	9	..	86
3	3	2	3	*	7	..	3	5	5	5	5	10	5	..	52
4	3	3	3	*	6	..	3	6	5	5	5	10	5	..	54
5	3	3	3	*	6	..	10	14	10	10	10	12	9	..	90
6	3	3	3	*	6	..	3	5	5	5	5	10	5	..	53
7	3	3	..	*	6	..	3	5	1	..	..	..	..	..	21
8	3	3	1	*	5	..	9	8	10	..	..	..	..	..	39
9	3	3	0	*	1	..	4	5	0	..	..	..	..	..	16
10	3	3	1	*	5	..	5	5	2	..	..	..	..	..	24
11	3	3	0	*	4	..	3	5	5	..	..	..	..	..	25
12	3	3	0	*	6	..	3	5	1	..	..	..	..	..	21
Noddies.															
No. 13	3	3	0	*	6	..	3	5	5	5	5	10	5	..	50
14	3	3	3	*	6	..	3	5	5	5	5	6	5	..	49
15	3	3	3	*	6	..	3	5	5	5	5	10	5	..	53
16	3	3	3	*	6	..	3	5	5	5	5	10	5	..	53
17	3	3	3	*	6	..	3	5	5	5	..	..	..	..	33
18	3	4	3	*	6	..	3	6	5	5	..	..	..	..	35
19	3	3	3	*	6	..	3	7	6	..	..	..	..	..	31
20	3	3	3	*	6	..	3	5	5	..	..	..	..	..	28
21	3	3	3	*	6	..	4	5	5	..	..	..	..	..	29
22	3	3	3	*	6	..	3	6	5	..	..	..	..	..	29
23	3	3	3	*	6	..	4	6	4	..	..	..	..	..	29
24	3	3	3	*	6	..	3	6	5	..	..	..	..	..	29

*Offered food, but would not eat.

The cages were kept in what had been the dining saloon of the ship (the *San Marcos* was formerly a Mallory passenger and freight boat; it had been converted entirely into a freighter). The canvas hoods of the cages were kept closed, except at feeding time, so that the birds had practically no chance to see anything which would give them their bearings. The following is a record of the releases *en route* to Galveston and at Galveston; the behavior of the birds on release follows:

May 20: Two birds released at night. Nos. 19 and 23 were selected as the best fed of the lot. No. 19 had 31 fish in all; 7 of them at 4 p. m. and 6 at 7 p. m. No. 23 had had 29 fish in all; 6 at 4 p. m. and 4 at 7 p. m. Both birds were in excellent condition, perfectly dry and clean. The tips of their wings were frayed for half an inch and the two lateral feathers of the tail. No. 23 was most active and the strongest of the noddies. It had never eaten of its own accord. No. 19 began to eat, without forcing, the day of release. The birds were released at 8^h 45^m ship time, and after all traces of sun had gone. The ship was headed a little north of west. The smoke was carried forward from the stack, which meant that there was a more than 10-mile breeze from

the east (against the birds). The sky was slightly clouded but not hidden. The ship was 372 knots from Key West and 220 from the nearest mainland, 88° west longitude and 26° 4′ north latitude, steaming almost due west. Both birds were placed upon the roof of the after-deck house. No. 19 was frightened and flew away at once. No. 23 refused to fly, vomited up its fish, plumed itself, and settled itself down on the roof, apparently for the night. I climbed up on the roof, whereupon it took fright. Both birds started to the northward and were lost sight of immediately.

Strange to say, No. 23, after a time, came back on board the ship and was captured by one of the sailors at 9 p. m. I examined the bird and found it dry and uninjured and released it again to the northward. About an hour later it came back on board again. It was frightened off by a sailor, but returned 10 minutes later. I decided to let the bird have its way and find out how far it would follow the ship. Remained on the deck until 7 in the morning, then flew eastward, alighting in the water half a mile from the ship. At this time the ship was 500 knots from Key West.

May 21, 8 a. m.: Released 4 noddies, Nos. 20, 21, 22, 24, and 6 sooties, Nos. 7, 8, 9, 10, 11, 12. The sooties all took flight at once. No. 12 first, when released, flew due east. The others went to the northeast, flying strongly. No 11 went down into the water. The noddies showed a tendency to hang around the decks, but finally all dropped astern and alighted in the water. The birds on release were all dry. The breasts and tail feathers of the sooties were badly soiled, but not caked with excrement. The tips of their wings were broken for half an inch and the lateral feathers of the tail were broken off in most cases. The day was clear, with a light breeze from the south. The position of the ship was as follows: 520 knots from Key West, 240 knots from Galveston, latitude 27° 52′ north, longitude 90° 3′ west.

May 21, 7 p. m.: Released two noddies, Nos. 17 and 18. The sky was completely overcast. A little wind. Both went astern and settled in the water. Heavy rains set in during the night and continued steadily during May 22. This unquestionably gave the birds a much harder task to accomplish than was ever put upon a homing bird before.

May 23: Clear morning with strong wind from east. *San Marcos* lying at wharf. Small sand island half a mile to northward. Fed remaining birds and turned them loose on deck. All were dry and clean and seemed to be in good health. Fed as shown in table 19. No. 13 was released first at $5^h\ 30^m$ a. m. It flew down to the sand island and alighted. The others sat on the deck for some time. The sooties then took flight, all reaching the sand island. The remaining 3 noddies flew down to the water and began to wallow in it near to the ship. They then arose and began to fly, flying with some gulls. At 7 a. m. they were still fishing and flying strongly and at that time were lost sight of to the westward. The 3 noddies, Nos. 14, 15, and 16, were observed again at $12^h\ 50^m$ p. m., fishing from the Mallory dock. Their flights were short, not more than 800 yards. They sat on the dock a good part of the time. The birds were lost sight of soon after the above record was taken and were not seen again.

RETURNS TO BIRD KEY FROM THE SECOND GALVESTON FLIGHT.

The record of the returns of these birds to Bird Key is given below in the form of field notes. The nests were observed several times each day. Every effort was made to keep the nests in good condition, to drive out strange birds, etc. We had been especially fortunate in choosing our locality; the nests were not molested very much by rats, gulls, and other enemies of the birds.

May 25: Noddy No. 22* was noted on the nest at $5^h\ 15^m$ a. m. Evidently the bird had returned the afternoon of the 23d in good condition. Took nest and began habit of sitter. Mate on nearby limb. This mate had not been any too faithful.

May 25: Sooty No. 9 noted on the nest at $2^h\ 45^m$ p. m.; might have arrived earlier in the day, since I was at Loggerhead from 9 a. m. to $2^h\ 45^m$ p. m. Bird in splendid condition. Red markings showed easily and clearly. Was sitting egg eagerly.

*Refer to Lashley's notes (p. 54) for time of release, weather conditions at time of release, distance from Bird Key, etc. The numbers of the birds given here are the same as those in Lashley's notes.

May 26: Noddy No. 20 noted at 10^h 30^m a. m.; was on limb near nest. Evidently had returned some time earlier in the morning. Poor condition. Seemed tired and worn. After a few days' rest bird began regular nesting activities.

May 27: Sooty No. 8 noted at 5^h 30^m a. m., in good condition; took nest and sat on egg.

May 27: Sooty No. 10 noted at 5^h 30^m a. m., in good condition; was sitting egg when noted.

May 27: Noddy No. 24 returned at 12^h 14^m p. m., very tired; took nest and was on nest when observed. Feathers in good condition. After a few days of rest normal nesting activities were resumed.

May 28: Sooty No. 12 found on egg at 9 a. m., in good condition; seemed quite fresh; was eagerly tending egg.

May 29: Sooty No. 11 on nest at 5 a. m., covering egg eagerly; condition good. This bird was very fierce when captured; had lost none of its fierceness; stood ground while I came up to the nest; mate not on hand. R must have come back at night, as it was not there at dark on the day before.

May 29: Sooty No. 1 noted on nest at 7^h 30^m a. m., in good condition; not sitting on egg; mate had deserted egg several days before.

May 30: Sooty No. 5 noted on nest at 11^h 35^m a. m., in good condition; sitting on egg.

June 2: Noddy No. 18 noted near nest at 8 a. m.; did not take nest; discovered by accident. The mate had been away, or rather idly sitting by, for several days. To-day and yesterday O had been on the nest more than usual. This aroused my suspicions, so I made a more thorough search. The bird seemed tired; did not fly away from limb as I approached, as rested birds always do. Possibly it did not return until to-day.

June 4: Noddy No. 14 noted near nest at 5 a. m. in fine condition; marking very clear; did not take nest at first, since nest mate was on. Yesterday I had decided that this nest had been deserted by O. I had often seen O nearby, flirting with a strange bird. Yesterday morning was actually taking food from strange bird in the nest. This morning both these birds were on and refused to let the marked bird on the nest; interesting to see if nest can be reëstablished. (R fought with the intruder for several days to get posession of the nest, but nesting relations were never resumed between R and O.)

June 7: Noddy No. 17 noted on nest at 7^h 30^m p. m. Bird probably had been back since June 5. Marking on breast had almost disappeared; hence my failure to note. This morning when I went out both birds were at the nest. This caused thorough examination, with the result that the marking was clearly recognized. Both were tending the nest. O had been very faithful all during R's absence.

The results of the second Galveston trip are put in convenient form in tables 20 and 21.

TABLE 20.

Date.	No. of birds released.	Distance from Key West (in statute miles).	Returned.
May 20, 8^h 45^m p. m.	2 (both noddies).....	418	None.
May 21, 8 a. m.....	10 (6 sooties, 4 noddies)..	585	8 (3 noddies, 5 sooties).
May 21, 7 p. m. (dark, rainy night).	2 (both noddies).....	720	2
May 23, 5^h 30^m a. m.	10 (4 noddies, 6 sooties)..	855	3 (1 noddy, 2 sooties).

The Galveston-Tortugas flight is deserving of some comment, as it shows the remarkable homing ability of these birds. It was not until this final trip that the conditions for caring for the terns approached those which are maintained for the homing pigeon. Even upon this trip the mistake was made of making the compartment too small. This tended to fray the feathers of the birds in such a way that the birds were probably somewhat hampered in their long

homeward flight. That more birds did not return from Galveston harbor is not to be wondered at when we consider the number of hawks which line the Texas shores. Our birds reached there tired out and probably remained on the shore for a time to rest; they would have made easy victims for birds of prey. It is probable that the hawks are in part to blame for the poor results we obtained from the Mobile flights.*

TABLE 21.—*Time required for return (individual birds).*

585-mile release.	720-mile release.	855-mile release.
No. 8, 5 d. 5½ h.	No. 17, 17 d. 2 h.	No. 1, 6 d. 2 h.
No. 9, 4 d. 6 h.	No. 18, 11 d. 13 h.	No. 5, 7 d. 6 h.
No. 10, 5 d. 21 h.	.....	No. 14, 11 d. 23½ h.
No. 11, 7 d. 21 h.		
No. 12, 7 d. 1 h.		
No. 20, 5 d. 2½ h.		
No. 22, 3 d. 22½ h.		
No. 24, 6 d. 4½ h.		

It would seem from all these experiments that where proper conditions are maintained the noddy and sooty can home from almost any distance up to at least 1,000 miles. Furthermore, the territory over which they take their flight apparently may be barren of any visual objects to which they previously may have established habits.

*In future experiments we suggest that they be released not less than 50 miles from any land.

BEARING OF THE PRESENT STUDIES UPON THE GENERAL PROBLEM OF HOMING.

We shall not here attempt to summarize our results in detail, but in the present volume there are two other papers (see p. 61 and p. 87) which throw some light upon the problem of distant orientation. We shall incorporate the main results of these papers with the results obtained from the present study and trace the bearing of the whole series upon the general problem of homing.

(1) Lashley (p. 61) has shown that the problems of proximate orientation are relatively simple and straightforward. On the island of Bird Key the terns make their adjustment to the nest, mate, young, etc., on the basis largely of visual habits. Kinæsthetic habits are involved, but to a less extent. There is no evidence of any remarkable or unusual sensitivity, nor of the functioning of any hypothetical sense-organ.

(2) The present paper seems to call for a separation between proximate orientation and distant orientation. We have never in the work on the homing pigeon been able to obtain sufficient evidence to reach such a conclusion. This arises from the fact that the pigeon has always been trained over a large part of its route. Such training allows the formation of numerous visual habits; hence all orientation may be looked upon as being of the proximate kind (we do not affirm such to be the case, however). The work at Tortugas, on the other hand, has placed us squarely before the problem (to take the limiting case) of accounting for the return of an untrained bird from a distance of 800 to 1,000 miles over a body of water which apparently can not offer any basis for controlling flight direction.

NOTE.—Two suggestions looking toward a simple explanation of the homeward flight of the terns have been made: (1) that the Hatteras birds simply followed the coast-line back to Key West, from which point they could see (?) Tortugas, and further, that they followed the coast-line south rather than north by reason of the fact that the former direction led them into warmer regions, which they instinctively seek; (2) that the Galveston birds and those released *en route* were aided by a well-marked water-current which sweeps around the coast of Texas, Louisiana, Alabama, Florida, and out past Tortugas through the Straits of Florida. This main current differs in color both from the surrounding and more stationary water, and from the return current, which runs nearer the coast line. We are indebted to Dr. W. J. Humphreys for the latter suggestion. Interesting as these suggestions are, it remains to be said that the explanation offered by (1) does not bear at all upon the more difficult Galveston flight. Dr. Humphreys' suggestion (2) meets with difficulty when we remember that some of the (successful) birds were released at night and that all of the birds were out from 4 to 20 nights. Furthermore, they had to win their way through rain, haze, and cloudy weather. Nor were all the birds which returned released at Galveston. They home equally well, no matter at what point between Galveston and Tortugas they are put down. Then, too, the difference in brightness or color between the current in question and the surrounding water depends largely upon the position of the sun with respect to the observer. Apparently the birds flew steadily (zigzagging, of course, for food) from morning to night (if not through the night), and during only a part of this time would the sun have a position such that the current could offer a difference in color or in white value. Even granting the existence of such data, the question as to why the bird follows the current towards the Straits of Florida rather than in the opposite direction is pertinent. There can be no question of any permanent temperature differences among these regions during May and June, nor can there be any question of chance, when the whole group of birds is considered.

(3) Mathematical considerations show that at such distances the goal can not possibly (directly) visually stimulate the bird, even granting absolute visual acuity and complete absence of haze, etc. The vertical height to which the bird would have to fly in order to overcome the sphericity of the earth is

so great that there could be no question of direct visual stimulation offered by the goal (which in this case we may consider to be the lighthouse, 151 feet in height) at distances greater than 100 miles (probably far less than this when we consider the fact that there is a continuous haze over the water). Even at 100 miles the bird must fly to a height of 0.9 mile in order to receive the light rays reflected from the goal.

(4) We have shown that the hazardous speculation of Duchâtel, which was designed to overcome the difficulties of the visual hypothesis discussed in (3), is without foundation in fact. The investigation reported on page 87, on the limits of spectral sensitivity in birds, shows clearly that neither the chick nor the pigeon is sensitive to infra-luminous rays. Furthermore, it was shown that the retina of the chick (and incidental confirmation has been found in tests on the pigeon) is probably less efficient in responding to monochromatic light of low intensity than the retina of the human being. The single exception to this statement is to be found in the extreme red, where the retina of the chick is slightly more sensitive than man's. If these facts are considered in connection with Johnson's work, which shows that visual acuity in the chick is far less than in man, we may very well accept with caution the popular view concerning the perfection of the organ of vision in birds.

(5) Our work has shown further, in the terns at least, that there is no special Spürsinn—special tactual or olfactory mechanism situated in the nasal cavity which may function in homing.

Our contributions are admitted to be negative in character. The difficulty of explaining homing by current theories is seen to be great, but, while admitting this, we do not suggest the assumption of some new and mysterious sense. The task of explaining distant orientation is an experimental one, which must yield positive results as soon as proper methods are at hand. Two lines of investigation offer hopeful results:

(*a*) The rearing of homing pigeons in a côte which is covered by a tent sufficiently large to offer the birds wing practice and to make proximate adjustments to their côte. Birds reared under such conditions may be sent out for a test of the possibility of homing in an untrained animal. After shipping the birds the tent which covers the côte could be removed. If they returned to the neighborhood the visual stimulus offered by the côte would arouse the old habit of entering it, etc.

(*b*) The same possibility is offered on a smaller scale by a slightly similar method. We could rear the birds in a wire-covered yard attached to a côte; then, as was done by Hachet-Souplet with the birds living in the traveling côtes (an experiment which certainly ought to be confirmed), we could tether individual birds to the top of the côte by cords which would permit a view *only of the neighborhood immediately surrounding the côte.* These birds would next be sent into a region which could not have stimulated their vision previously. Again, if the birds effected a return to the neighborhood of the côte, they would resume their old habits.

With these experiments upon homing, work upon the sensory equipment of the homing pigeon should be carried on. It is just possible that these animals possess on certain parts of the body (eye-lids, ear covering, oral cavity, etc.) sensitive tactual and thermal mechanisms which may assist them in reacting to slight differences in pressure, temperature, and humidity of air columns.

1

2

METHOD OF MARKING NEST AND BIRD FOR HOMING.

Fig. 1. Noddy nest tagged. Four horizontal bars are painted in oil across the bird's head.

Fig. 2. Sooty nest tagged. Large daub of oil paint is placed on the bird's white neck.

NOTES ON THE NESTING ACTIVITIES OF THE NODDY AND SOOTY TERNS.

By K. S. Lashley.

ORIENTATION IN THE NEST LOCALITY.

In the report of his extensive studies of the activities of the noddy and sooty terns* Watson raises the question of the method by which the birds recognize their nests and young and records the results of a few experiments bearing upon the problem. The terns breed in great numbers upon Bird Key, an island in the Tortugas group somewhat less than 5 acres in area. Their nests, of which there were more than 10,000 in 1908, are in may cases closely crowded together, as many as 30 sooty nests being found in an area of 100 square feet, and the nests and eggs are almost indistinguishable to the human observer. Indeed, the island suggests a city of 10,000 houses, all much alike, unnumbered and set down at random, without streets or definite landmarks. The birds choose their own nests, without error, from among hundreds of similar ones, and under normal conditions never show the slightest hesitation in making their choice.

In testing the sensory factors involved in nest recognition, Watson found that he might change the appearance of the nest and egg without disturbing the birds in the least. Very great changes in the appearance of objects near the nest did not alter the quickness and accuracy of orientation. When, however, the slightest alteration was made in the horizontal position of the nest, when it was moved only a few inches to one side, the birds were much confused, refused to occupy the nest in its new position, and reacted positively to the original nest site even when no trace of the original nest remained. Many observations of such behavior led Watson to conclude:

> In the case of both the noddy and the sooty, the nest locality is the important factor, the nest itself being reacted to by virtue of its location within this locality. Since environment can be greatly changed without altering the bird's accurate adjustment to the nest, it is evident that if the adjustment is made in terms of visual data, the visual environment which serves as the stimulus must be complex and have a wide extension. I am not prepared to admit, from the above experiments, that adjustment takes place in terms of vision alone.

In view of the remarkable ability of the terns to return to their nests from great distances the problem of the sensory factors involved in their recognition of the nest locality and orientation in the neighborhood of Bird Key becomes of considerable importance as offering a possible aid to the understanding of more distant orientation.

At the suggestion of Professor Watson I attempted to gain further evidence upon the problem of proximate orientation in the terns during a six weeks' residence on Bird Key. The work has not furnished any clue to the mechanism of distant orientation, but the details of the birds' behavior in finding and

*John B. Watson. The Behavior of the Noddy and Sooty Terns. Papers from the Tortugas Laboratory of the Carnegie Institution of Washington, Publication 103, 1908. In this is included a general description of the activities of the terns during the nesting season. The reader is referred to the paper for figures of the birds at different ages and for a more complete description of the behavior of the birds at different times during the nesting season than is given here.

recognizing their nests seem worth recording as exemplifying the extremely complicated habit-systems which wild animals may develop under a normal environment, and, also, as showing that, whatever the mechanism of distant orientation may be, proximate orientation is readily explainable in terms of visual and kinæsthetic habits.

I wish to express my indebtedness to Professor Watson for much helpful suggestion and coöperation in the work, and to Dr. Alfred G. Mayer, of the Carnegie Institution of Washington, for his kindness in placing the facilities of the Marine Biological Laboratory at my disposal.

Plate 7, figs. 1 and 2, show roughly the character of the nesting areas of the sooties and noddies. Detailed descriptions of the nests and eggs have been given by previous writers, and the following brief notes are included only for the sake of a clearer presentation of the experimental control of orientation.

The nest of the sooty consists usually of a shallow depression in the sand, of about the depth and diameter of a large saucer. The birds hollow it out by a sort of wallowing movement, turning around and around and pushing the sand away with their breasts. In this depression a single egg is laid. Rarely two eggs are found in a single nest, but whether both are laid by a single bird, or one

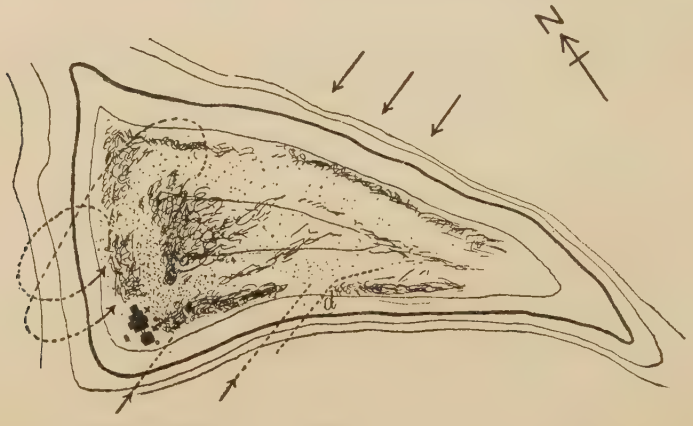


FIG. 2.—Diagram of Bird Key. Buildings in black at the left. The chief nesting areas are represented by stippling. Solid arrows represent direction of prevailing winds. Dotted lines show chief paths followed by birds in approaching the island.

is stolen from a neighboring nest, is uncertain. In 1913 the greater number of sooty nests were made on the western end of the island. Their position and relative number are indicated by the stippling in figure 2. The nests of the noddies are loose masses of sticks piled among the branches of the low shrubs which cover the greater part of the island. They are frequently lined with bits of coral and shells, and, like the nests of the sooties, rarely contain more than a single egg.

The key upon which the nests are built is roughly triangular, about 500 yards in length and half as broad. Its principal features are shown in figure 2. Its greatest elevation is not more than 6 feet. The key was formerly overgrown thickly with bay cedars, but the greater number of these were killed by the hurricane of 1910 and only a few living cedars remain, for the most part

near the center and western end of the key. These grow to a height of 5 feet and make many irregularities in the profile of the key which are probably important as landmarks for the birds. Where the bay cedars have been washed out the sand is covered by low fleshy herbs resembling the common tumble-weed. At the beginning of the nesting season large numbers of these weeds were torn up and piled along the beach to give the sooties nesting space in the center of the island.

At the western end of the island are some buildings, the warden's house and the old detention hospital, which are the most conspicuous objects upon the island. The group of buildings is about 50 feet in length and has a maximum height of 15 feet. Three cocoanut palms (marked x in figure 2) grow near the buildings. The buildings, the palms, the clumps of bay cedars, and the piles of brush along the beach present extremely complex visual stimuli to the birds coming home from sea. Even at the most isolated nests the environment presents a countless number of possible visual stimuli and it is never practicable to control more than a very few of the stimuli which may be potent factors in the birds' orientation. Long-continued study of a single nest is prevented by the fact that the birds soon become hopelessly confused by the changing conditions of the experiment and cease to react definitely to the nest or nest environment. The situation at every nest presents a distinct problem and in no case has it been possible to reconstruct the entire process of orientation from the data obtained at a single nest. The relation between particular stimuli and the orientation of different birds has been determined, and a combination of the results of observations at many nests gives a fairly complete picture of the mechanism of orientation in the immediate neighborhood of the keys. The experiments recorded in the following pages are typical of a large number carried out at other nests and will serve to illustrate the chief phases in the recognition of the nest and nest site.

EXPERIMENTS WITH SOOTIES.

RECOGNITION OF THE NEST SITE.

The sooties become accustomed to the experimenter's presence very quickly and often become so tame as to interfere seriously with the experiments, refusing to be driven more than a few feet from the nest. After they have been disturbed a few times their return to the nest is very prompt. The accompanying table gives the time elapsing from the moment when the brooding sooty was driven from the nest until it had again covered the egg. The time given is based upon the average of 6 birds during 5 successive trials at intervals of 5 minutes. The average time which a tame sooty requires to rise from the nest, fly 15 or 20 yards and return, is 12 seconds. This is not appreciably changed by the substitution of a strange egg for the one in the nest, or by the filling in or remodeling of the nest by the experimenter, so long as the egg remains in nearly its original position. A very slight change in the lateral position of the egg, however, causes a very great increase in the time required by the bird to return to the nest and begin to brood the egg. In adjusting to such changes the sooties show a very accu-

Trial.	Time (seconds).
No. 1	56
2	25
3	30
4	18
5	12

rate localization of the original position of the nest, even when all traces of it have been completely obliterated. The following notes give a description of a typical instance of adjustment to a lateral displacement of the egg.

EXPERIMENT 1.

10h 50m a. m. I drove a sooty from her nest, obliterated the nest, and made a new one with a noddy egg in the same place. In 1 minute the bird returned and in 2 minutes covered the egg, after driving away two trespassers.

10h 53m. I obliterated the old nest and made a new one, 1 foot south of the first position. The bird alighted at the old nest and turned around as if to adjust the egg. She then caught sight of the egg in its new position and went over to the new nest, then returned to the old one. She then made 4 more trips back and forth between the new nest and the original nest site, and finally settled down at the latter, hollowing out the sand to form a shallow nest. After a minute she went to the egg, adjusted it, and settled down on it. Another sooty attacked her and she drew back from the egg without defending it, looking back to the old nest site. She next went to the old nest, then back to the egg, which she covered for 30 seconds. After one more trip to the old nest she seemed reconciled to the egg in the new position. The total time required for this readjustment was 10 minutes.

11h 5m. I drove her away again. She returned in 1 minute, alighted between the first nest position and the new one, went first to the egg, then to the old nest site, then back to brood the egg. Two minutes later she paid a brief visit to the old nest.

11h 10m. Driven away again, the bird alighted near the old nest site and ran directly to the egg. After 30 seconds she visited the old nest site, and again after 2 minutes. She was driven away 6 times during the next half hour and at the end of this time showed an almost perfect adjustment to the changed conditions, returning to the egg in 15 seconds. The egg was then returned to its original position. The bird accepted it at once and paid not the slightest attention to the new nest which she had just been occupying.

In this case the substitution of noddy egg and artificial nest for the original egg and nest did not affect the bird's reaction in the least.

EXPERIMENT 2.

In another case the egg was taken from a nest, a piece of black cloth 3 feet square was spread over the nest, and the egg was replaced on top of the cloth. The bird alighted in a few seconds and walked immediately to the edge of the cloth. It showed a good bit of hesitation in stepping on the cloth, and walked around it, looking at the egg for many seconds. In less than a minute, however, it ventured upon the cloth, seemed to lose all fear immediately, ran to the egg and covered it contentedly. The total time required for adjustment to the new situation was less than a minute.

Other instances of similar indifference to the appearance of the nest and egg are recorded in later experiments and make it clear that, except under unusual conditions, the individual characters of the nest and egg do not play any important part in orientation. The birds react positively to any nest at the chosen site. In contrast to this the birds are almost always very greatly disturbed by a lateral displacement of the nest of only a few inches and react to the original nest site much as did the bird described in experiment 1. The time required for readjustment to such slight changes varies greatly with different birds, even under quite similar conditions.

EXPERIMENT 3.

The same birds whose average time of adjustment to disturbance is given on page 63 were used for a comparison of the times of adjustment to a change in the position of the nest. The nests were similarly located in open, rather thickly populated areas, but so far apart that no two of the birds under observation were disturbed at the same time. In each case the nest was obliterated, a new nest was constructed 1 foot from the old nest site in the

direction most free from other nests, and the egg was placed in this artificial nest. The birds were allowed to find the new nest and settle down quietly over the egg each time before being frightened away again. Table 22 shows the time required by each bird to readjust to the new situation. The birds were driven away at intervals of 5 or more minutes.

TABLE 22.

No. of trial.	Time in seconds.					
	Bird No. 1.	Bird No. 2.	Bird No. 3.	Bird No. 4.	Bird No. 5.	Bird No. 6.
1	240	480	300	(*)	30	100
2	15	60	270	...	20	100
3	120	240	30	...	90	50
4	150	120	40	...	(†)	50
5	15	65	55	...	...	40
6	50	75	30	...	...	20
7	15	300	36	...	...	30
8	10	90	...	...	...	50
9	10	30	...	...	...	40
10	15	30	...	...	...	...

*Did not find egg in half an hour. †Undisturbed by change.

In each case but one the birds were greatly confused by the change in the position of the nest and all showed a tendency to settle down at the original nest site. As it was clear that the nest itself was not the stimulus to the localization of this area, an attempt was made to control the immediate surroundings of the nest. The following experiments with objects near the nest give some insight into the mechanism of nest recognition:

EXPERIMENT 4.

In making their nests the sooties avoid the beach, very rarely going beyond the fringe of bushes which marks the highest tide level. A few nests were made, however, at some distance from the bushes upon the sloping sand above high-tide mark. One of these on the western shore, well removed from any other nest, was obliterated and replaced by two artificial nests, one 6 inches to the east, the other 6 inches to the west of the original site. The egg from the original nest, which was rather light in color, was placed in the western nest and another, marked with dark blotches, in the eastern one. The sand sloped from east to west at about 15 degrees.

Two minutes after these adjustments were made the sooty returned and, after inspecting both nests, chose the strange egg in the eastern one (7^h 20^m a. m.). Half a minute later she got up, went over to the western nest, and covered it. Her mate came and examined the eastern nest. The first bird, attracted by his movements, went back to this nest and settled down there. Her mate then flew away. She soon became dissatisfied with the eastern nest and made several trips back and forth between the two. The visual stimulus of the unoccupied nest seemed to furnish a stronger impulse than the tactual stimulus of the nest occupied. At 7^h 25^m a. m. the bird stood for some time between the two nests, looking first at one, then at the other, and finally chose the eastern one. She was driven away, returned, and took the eastern nest. Half an hour later she was driven away again; she returned, again took the eastern nest, and paid no further attention to the other.

The two nests were then obliterated and replaced by two others at the same distance from the original site, but now north and south of it. The bird returned and chose the southern nest, paying no attention to the other. Control tests showed that in this case the position of the observer did not influence the choice of the nest.

One egg was then placed in the original position, the other in a new nest 3 inches to the eastward of this. The bird returned and chose the western nest, but soon left it for the

other. She was driven off five times and each time returned and took the eastern nest. As she always approached from the southeast it is probable that the direction of approach determined the choice.

The western nest and egg were next moved 11 inches to the eastward, 8 inches farther from the original nest site than the former eastern nest, which was left undisturbed. The bird returned and took the new nest. The western nest was again shifted to the eastward. The bird returned and again took the eastern nest. By shifting the two nests alternately it was possible to move them 4 feet to the eastward without confusing the bird. Each time she chose the eastern nest in preference to the former eastern one which she had just occupied. But when the nests were brought near the bushes the bird began to show signs of dissatisfaction, finally returned to the original nest site, and settled down there. After a time she got up and went back to the two nests. This time she covered the western one. The nests were then shifted, a foot at a time, to the westward, the bird each time choosing the western one, until they were near the water, 4 feet west of the original nest site. Here the bird again became dissatisfied and returned to her old nest. The eggs were finally placed 8 inches east of their first position and left for half an hour. At the end of this time the bird had rolled one of them to the original nest site and was brooding it there.

In approaching the nest the bird always flew along the shore skirting the fringe of bushes, then alighted about 6 feet southeast of the nest and walked directly to it. At various times during the experiment she started from the alighting place straight toward the original nest site, but turned aside to the new nests before reaching the old position. The movements of her head indicated that she took her bearings from some object to the northward. What that was I could not discover.

It was found here that recognition of a nest is to some extent dependent upon the relative positions of the nest and other objects in the neighborhood, in this case an artificial nest. Other factors not brought out by the experiment seem, however, to play a still more important part in orientation. The constant alighting place and path to the nest furnish a clue to other factors in orientation which appear more strikingly in other experiments.

Experiment 5.

A nest in an open space (at *a* in figure 1, plate 7) near a short stake (*f*) was obliterated and replaced by two nests a foot apart. The bird chose the northern egg. Both eggs were moved a foot to the northward. The bird alighted and chose the northern egg without hesitation. Her mate came and covered the southern one. Both were driven off. The first returned and alighted at a place 4 feet to the eastward (*g*), then walked for 3 feet straight toward the original position of the nest. When still a foot away she seemed to catch sight of the eggs in the new position, turned sharply from her course, and went straight to the northern egg (*d*). She soon became dissatisfied, went back to her alighting place (*g*), and returned to the northern nest, but this time directly. She then went to the southern nest (*b*), returned to the northern, and covered the egg. Her mate came back and again took the southern nest.

Both nests were again moved, with the stake, 2 feet to the northward. The first bird alighted as usual near the bushes (*g*), walked to the original nest site (*a*), looked about, and went directly to the new southern nest (*c*). Her mate came and walked about the old nest site. She got up and went to him, then returned to the nest.

The stake was then moved back to its first position (*a*), the nests being left undisturbed in the northern position. The first bird came to her alighting place and started directly toward the new site, then seemed to catch sight of the stake and turned toward it, then back to the alighting place, toward the stake, back again to the alighting place, again toward the stake, then to another nest, where she seemed to get her bearings, toward the new nest, toward the alighting place, then finally to the new nest. The path followed is indicated in figure 3, and shows clearly the struggle between the motives offered by the sight of the stake and that of the egg.

The egg and stake were next placed 2 feet south of their original position (at *h*, in figure 1, plate 7). The bird returned to her alighting place (*g*), looked about, and went straight to the new nest. The egg and stake were then moved to a position near the alighting place (*e* in figure 1, plate 7). The bird alighted 2 feet north of the stake, went to it, and covered the egg without hesitation.

The stake and egg were moved 2 feet to the westward (*f*) and another egg was placed where they had just been (*e*). The bird returned to her alighting place, examined the egg and stake (*f*), then covered the egg in the former position (*e*). She was driven off and returned to her alighting place immediately. Another bird alighted near the unmarked nest (*e*) and disputed her possession of it, so she went to the egg by the stake and covered it. A moment later she got up and went back to the unmarked nest. The strange bird made way for her and she covered the egg there. This is the position in which she is shown in plate 7. It is in the direct line between the alighting place and the original site (*a*). The egg and stake were next placed between this position and the original nest site. The bird continued to occupy the unmarked nest. The egg and stake were returned to their original position, but the bird did not again take them.

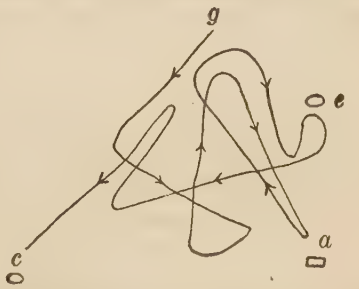


FIG. 3.—The path followed by the sooty in experiment 5 when the egg (*c*) was moved away from the stake (*a*). The reaction to the 4 chief visual stimuli of the region is shown.

This series of observations offers several interesting points:

(1) In comparison with the sooties in crowded localities, the bird under observation showed a remarkable plasticity. Attempts to duplicate the experiment in crowded districts failed completely.

(2) The constancy of the place at which the bird alighted is very striking. How this place was recognized could not be determined, but it was near an open place in the bushes which offered many more visual criteria than did the immediate surroundings of the nest. The certainty with which the direction was taken from this position to the nest is also unexplained. It was to a certain extent independent of the appearance of the immediate vicinity of the nest. The question of orientation from the alighting place will be taken up later.

(3) The relative force of the egg and stake in determining behavior gives some insight into the bird's habits with respect to the nest environment. The stake had been planted 2 weeks before to mark the site of another nest, at that time destroyed. In 2 weeks it came to occupy an important place in the bird's recognition of her nest, perhaps taking the place of the obliterated nest in her system of habits. That it was not the fundamental factor in locating the nest is shown by the fact that after a few failures to reach the nest by going to the stake the bird paid little further attention to the latter.

EXPERIMENT 6.

A nest in a rather crowded area was chosen for this experiment. It was 8 inches north of a stake which had been planted a week before to mark another nest, and near the center of a triangle (figure 4), the northern side of which was formed by a line of very dense weeds (*c*), the western by a very dense clump of weeds (*f*) and the stem of a palm leaf (*d*), the southern side (*a–g*) being open. The stake shown in figure 4, at the right of the nest (*b*), was originally at *h*. I drove the bird from the nest—she circled above the island and came back immediately, alighted 6 feet to the eastward of the nest, and walked almost straight to it (along the line *a–b* in the figure). This was repeated twice, her path in returning being each time the same.

The stake at *h* was by far the most conspicuous object near the nest, and to test its effect upon the bird's reaction I moved it from its position south of the nest to an equal distance north of the nest. I also constructed a new nest, north of the stake at the position marked *c*. The relation between this nest and the stake was now the same as the relation of the nest at *b* and the stake had been before the change. The bird's own egg was placed in the nest (*c*) and a strange egg was placed in *b*. The bird returned by the path from *a* and took

its own nest at *b* after some hesitation. The change in the position of the stake was a disturbing factor, but was not sufficient to determine the choice of nests.

The line of weeds forming the northern boundary of the triangle was next moved a foot to the northward. The bird returned by its customary path without the slightest hesitation. This landmark played no part whatever in orientation.

The weeds were returned to their original position and the objects forming the western border of the triangle were moved a foot to the northward. This brought the clump of weeds and palm leaf, originally at *g* and *f*, to the position shown in the figure. The bird alighted far to the south and passing around between *h* and *e* took the northern nest at *c* and settled down quietly. She was driven away and the palm leaf was returned to its original position, *f*. The bird returned and occupied the northern nest, *c*, for 5 minutes. She was again driven away and the clump of weeds was returned to its original position, *g*. The bird returned and took the southern nest, *b*, without hestitation. The leaf and weeds were moved back to *d* and *f*. The bird chose the northern site; they were returned to the southern position, the bird returned to the northern nest, adjusted the egg, got up, walked to the southern nest, and covered the egg there.

FIG. 4.—Environment of the nest described in experiment 6.

The weeds were moved northward to *f*, the leaf was not disturbed. The bird chose the southern nest. The leaf was moved to the northward. The bird chose the northern nest. Leaf and weeds were moved to the southern position; the bird came to the northern nest, hesitated for 30 seconds, then chose the southern nest. This shift was repeated many times until it was quite certain that the bird always took the same position with respect to the two objects. Finally they were removed entirely. The bird showed signs of confusion, occupied the two nests alternately for several minutes, and finally settled down in the southern nest.

The weeds to the northward were cleared away and the nest, *b*, was moved 18 inches to the northward of *c*, to a position at the extreme right of the figure. The leaf and weeds were likewise shifted to the northward, so that the original relations of the nest and two objects were now duplicated, nearly 3 feet from the original position. The bird returned and covered the southern egg at *c;* 3 minutes later she got up, went to the new nest, adjusted the egg, and covered it. I watched her for half an hour longer, during which time she remained contentedly on the nest; 4 hours later her mate had returned and taken the southern nest, which he occupied during the whole of the following day.

From this rather long account it is clear that the final orientation was determined wholly by the visual stimulus of two objects in the neighborhood of the nest. Neither the palm leaf nor the clump of weeds alone was sufficient to determine the choice of the nest, but both together called forth a definite reaction, regardless of all other objects in the vicinity.

Watson found that considerable changes could be made in the vertical position of the nest without affecting the bird's reaction in the slightest. His experiments have been repeated, but always with negative results. The following notes illustrate the type of reaction obtained:

EXPERIMENT 7.

A nest near that described in experiment 5 was placed in a pan of sand, 1 foot square and 2 inches deep, so that it was raised 2 inches above its former level. The owner of the nest always alighted 6 feet to the eastward and walked by a somewhat zigzag path to the nest. When the pan of sand was first put in place she approached, walked around it, and inspected it carefully. She next stopped, peered about in all directions, then stepped up to the higher level. She did not cover the egg, but walked about in the pan and finally backed over the edge and fell out. She spent some time standing near the pan, then stepped up on it again, and after 8 minutes covered the egg. Half an hour later she was driven away. She returned at once, spent 2 minutes in preening her feathers, then stepped up into the pan, and covered the egg immediately.

The pan was next raised to a height of 6 inches from the ground. The bird returned to her alighting place, walked toward the pan, hesitated, walked around it several times, but made no attempt in half an hour to get upon it. With the pan in this position she could not see the egg from the ground. The pan was then lowered to a height of 4 inches. The bird came from her alighting place to the pan, climbed into it with difficu'ty, putting one foot in it and fluttering up, and covered the egg in 2 minutes. The nest was left in this position until the following day, when the experiment was resumed. The bird was then driven off several times and each time returned to her usual alighting place, walked to the edge of the pan, then fluttered up to the egg. The nest was raised to 5 inches and was taken immediately. It was next raised a foot from the ground. The bird walked around it for 5 minutes before flying up and covering the egg. It was left in this position for an hour and was then raised to a height of 2 feet. The bird returned to her alighting place, walked to the foot of the post supporting the nest, and seemed very much puzzled by the new condition. She walked round and round the post, looking up at the nest, but in 10 minutes made no attempt to fly up to the pan. The pan was lowered to 6 inches and was occupied immediately.

The pan and its support were moved 3 feet to the westward and fixed at the same height. The bird came to the former nest site and wandered about for 10 minutes, but did not seem to see the nest in the new position. The nest was next moved 6 feet to the eastward of its original position and fixed at the bird's alighting place. The bird came to the ground beside the nest and fluttered up and covered the egg immediately without a sign of dissatisfaction. The pan was returned to its first position and fixed at a height of 6 inches. It was accepted at once.

Two days later the same bird was upon the nest. The egg had hatched in the meantime. The pan was raised to the top of a post 2 feet above the ground. The bird walked around the post for half an hour without seeming to look up at all. She then flew away and was not seen for the remainder of the day. The next day she was again in the nest. When driven off she returned and alighted directly upon the nest.

The pan was raised to a height of 5 feet. The bird came to the ground and walked around under the pan for several minutes, looking up. Then came the most interesting part of her entire series of reactions. She flew up to a height of 2 feet and went through the motions of alighting, just as though the pan were still at this height, then dropped back to the ground. She repeated this five times in exactly the same way, then walked about for a few minutes, looking up, and finally flew up to the pan and covered the nestling. She was frightened away again, returned to the ground, and flew up to the pan immediately.

This experiment brings out three facts quite clearly. First, adjustment to changes in the height of the nest is not made any more readily than to changes in the horizontal position. Second, shifting the nest along the pathway toward the alighting place disturbs the birds less than a change in the position of the nest in any other direction. Third, there is strong tendency to replace visual motor reactions by habits based upon kinæsthesia, resulting in reactions like those shown by the rat in the adjustable maze.

Clearly, in all these experiments the birds were influenced to some extent in the recognition of their nests by visual objects in the immediate neighborhood. Such objects were other nests, débris which had been in place since the nest was made, and objects recently added to the situation. It appears that long

familiar objects influence orientation more than those added after the laying of the egg. The birds pay more attention to the nests of others than to any other class of objects.

In the case of some other nests, particularly those in crowded areas, I could find nothing in the immediate neighborhood of the nest which seemed to influence the birds' orientation. At one time a nest was shifted 1 foot from its original position and practically every object within a radius of 3 feet was moved for the same distance in the same direction. The sand in this area was smoothed out to change its appearance. Thus the original environment was duplicated with 1 foot lateral displacement. The bird came back to the position of the orignal nest, now completely obliterated, and gave nearly the same set of reactions as that of the bird described in experiment 1. In this case the recognition of the nest locality was upon the basis of some other stimulus than that provided by the immediate environment. The same was in part true of the nests described in experiments 4 and 5.

THE ALIGHTING PLACE.

The clue to the method of orientation in such cases is given by the behavior of the bird described in experiment 5 in passing from her alighting place to the nest. It seemed that this bird took its direction from some objects at the alighting place and depended upon this place for re-orientation when confused by changes in the position of the nest. The following observations bring out the importance of the alighting place and the path from this to the nest.

Experiment 8.

At the edge of the beach two piles of brush were thrown together in such a way as to leave a small over-arched passage extending half way through between the piles and opening on the beach to the westward (*z*, figure 5). A pair of sooties had their nest at the inner end of this passage. The bird studied alighted always at the southern end of the brush pile, walked around the southwestern corner, where there was another nest (*x*), and then followed the western side of the pile to the opening of the nest. The path was complicated by a dead bay-cedar branch (*y*), which projected from the face of the pile and forced the bird to stoop and turn out to the left. The bird was seen to follow this path accurately in five successive trips to the nest. I broke off the branch and removed it, leaving a clear path from *x* to *z*. On the next two trips to the nest, *the bird went through exactly the same movements of stooping and turning out which had been required by the presence of the dead branch.*

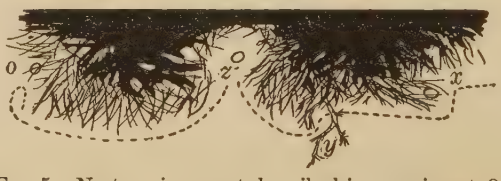


Fig. 5.—Nest environment described in experiment 8. *z*, nest of bird studied; *x* and *o*, other nests; *y*, branch of bay-cedar bush.

I moved the egg out from under the brush to a point a foot west of its original position. The bird returned immediately and went to the former position of the nest. She was disturbed by the absence of the egg, came out slowly, and returned to the southwest corner of the brush pile. She looked at the nest there, then turned and ran quickly along her usual path to her own nest. This was repeated four times. The bird's movements in going from the nest site to the corner of the brush pile had every appearance of hesitation and uncertainty, a marked contrast to her behavior after she reached the point from which the

neighboring nest was visible. The sight of this nest seemed in every case to furnish the basis for orientation, for the bird always hastened back to her own nest after looking at the neighboring one. After 15 minutes she found the egg in its new position and soon covered it, to all appearances satisfied; 5 minutes later she was driven away, returned to the southern end of the brush pile, and walked to the old nest site by the customary path. She settled down in this position for a few minutes, then became dissatisfied, went over to the egg, adjusted it, and covered it.

The path from x to z was then blocked by piling brush at y. The bird alighted near the nest at x, started along the path, came to the pile of brush, and seemed to be completely lost. She wandered about for a long time and finally came to the nest (o) at the other end of the brush pile. She crawled into the brush and inspected this nest, then turned and scurried around to her own nest at z as fast as she could run. The nest at o, like that at x, served as a landmark.

Under normal conditions a nest near the alighting place furnished the bird a clue to the direction of its own nest and the path to the latter was followed almost wholly by a series of kinæsthetic-motor habits. The bird was able to re-orient from other visual stimuli when the usual method was prevented.

EXPERIMENT 9.

An isolated nest upon the beach, just at the foot of a steep sand bank covered with dead bay-cedar bushes, was chosen for observation. The environment of the nest is shown roughly in figure 6. The nest was at the position marked d. The brooding bird was driven off several times and her return noted. She never alighted upon the beach, but always upon the top of the bank, among other nests, at a distance of more than 8 feet from her own (at a or b). From this point she walked to her nest, following a winding path about 12 feet in length. This path led through a clump of weeds, straight down the face of the bank, around a dead bay-cedar bush, and along the foot of the bank for 4 feet to the nest. Along the path were five other nests, which were always avoided carefully. These nests were on the face of the bank; there were no other nests at its foot.

FIG. 6.—Nest at the foot of a sand bank, with the path from the alighting place shown by the dotted line.

Moving the nest along the foot of the bank for a distance of 2 feet in either direction did not seem to affect the bird's behavior in the least. A piece of canvas was placed against the face of the bank so as to cover the bird's path from the top to the foot. The canvas was nowhere nearer than 5 feet to the nest and did not cover the bird's alighting place at the top of the bank. The bird returned, seemed frightened, but in 5 minutes alighted and walked upon the canvas. She started down the bank in the right direction, but became confused as soon as she got upon the canvas, and returned to her usual alighting place. She made several more false starts, became accustomed to the canvas, and walked upon it without hesitation, but never walked the full distance to the foot of the bank. She went a short distance to one side of the canvas and reached one of the nests bordering her path (c). She looked at this for a moment, and then turned and ran rapidly in the direction of her

own nest, but was again interrupted by the canvas. After a great number of failures she finally reached the foot of the bank, seemingly by chance, half an hour after the canvas was placed in position, and from this place she followed her customary path to the nest. An hour later she was driven from the nest (she probably made many trips to and from it during this interval). She returned to her original alighting place at the top of the bank and flew from it across the canvas to the foot of the bank, then walked to the nest.

In this case the alighting place was far from the nest. Blocking the path between the alighting place and the nest was sufficient to destroy orientation. The immediate neighborhood of the nest was reached by chance, but was recognized at once. Readjustment to the changes in the path followed very quickly.

From many observations I am convinced that practically every sooty has a special alighting place and path which is invariably followed in the return to the nest under normal conditions. In some cases the path seems unaccountably complicated. Figure 7 shows the path followed by one bird in 10 successive trips to the nest. It passed its own nest, then around a large clump of weeds, back past its own nest at *b* to inspect the nest at *c*, and only then occupied its own nest.

In many cases it is evident that after orientation is gained the path to the nest is determined largely by motor habits irrespective of the immediate visual stimuli. Little direct evidence upon the recognition of the alighting place has been gained, and the attempt to trace back orientation beyond this point must be postponed until data from experiments with noddies has been presented.

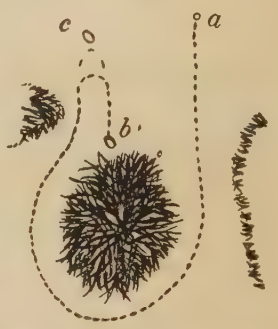


FIG. 7.—Complicated path from the alighting place (*a*) to the nest (*b*).

EXPERIMENTS WITH NODDIES.

THE NEST AND PERCH.

As in the case of the sooties, Watson found that the noddies locate their nests readily when the appearance of the surroundings has been completely altered and that the substitution of an artificial nest and egg for the originals does not disturb the birds in the least. He found also that very slight changes in the position of the nest confuse the noddies and that after such changes the birds still react positively to the old nest site. The nests of the noddies are always built upon the bay cedars and any great change in the appearance of the nest and environment involves not only a great deal of labor but also the destruction of many other nests. For this reason no great number of experiments upon the nest recognition of the noddies were carried out. Such experiments as the following, together with a large number of observations upon the flight of the birds to their nests, give, however, a fairly complete picture of the mechanism of orientation.

EXPERIMENT 10.

A pair of noddies had built their nest in a dead cedar bush near the smaller of the palm trees shown in plate 7, figure 2. When both birds were at the nest, one or the other was always to be seen perched on the middle of a low leaf of the palm. One of the birds is shown in figure 2, *c*. The nest was torn from its position and fastened on top

NESTING LOCALITIES OF NODDIES AND SOOTIES.

Fig. 1. Typical nesting area of the sooty.

Fig. 2. The environment of the noddy nest described in experiment 10.

of a stake, *a*, 3 feet farther from the perch, *c*, than its original position. At this time only one bird was present at the nest. It was frightened away and did not reappear for some minutes after the change was effected. It was first identified again as it hovered over the old nest site. Thence it flew to the perch, *c*, then back to the first position of the nest. This was repeated several times.

Without paying the least attention to the nest at *a*, the bird next flew to the large palm, *b*, circled close around its stem, and darted back to the perch. Without alighting here it turned and dropped quickly to the bushes where the nest had been. This was repeated many times until it seemed quite certain that the sight of the large palm furnished the clue to the position of the perch and that the change in direction of flight from the perch to the nest was effected largely in terms of kinæsthesia.

Three points were brought out by these observations. First, the existence of a well-localized perch corresponding to the alighting place of the sooty; second, the use of other very conspicuous objects (in this case, the large palm) as points from which orientation is gained; third, the importance of kinæsthetic-motor habits even in determining the direction of flight. The perch seemed to be recognized partly by its appearance and partly by its relation to the neighboring palm tree.

Experiment 11.

The nest of a noddy, upon a low clump of bushes, 30 feet from any other noddy nest, was torn from its position and placed 2 feet away at the same level upon the same group of bushes. The nesting bird returned, circled about the nest, and alighted upon it immediately. She covered the egg at once, but seemed a little uneasy. As the nest was not very secure in its new position, its unsteadiness may account for the bird's restlessness.

The nest was next fastened on top of a stake 3 feet above its original position. The bird came back from the westward toward the old position of the nest, caught sight of the new arrangement, and swerved out of her straight course. She circled the nest three or four times, always keeping below the new level at the same height at which she usually flew above the bushes. She then flew out to sea, turned, and came back to the shore. The roof of a small porch on one of the old buildings offered a convenient perch about 20 feet east of her nest and she flew up to this. The most interesting part of her whole reaction occurred here. Instead of alighting on the roof, she hovered beside one corner of it for a moment, then turned and darted unhestitatingly to the old nest site, 3 feet below the nest. She repeated this time and time again, flying to the corner of the porch roof, back a foot above the edge of the bushes past the nest, up into the air, and back to the porch roof. She showed no disposition to alight upon the nest after 15 minutes.

The nest was next lowered to a foot above its original level and 2 feet to the northward of its first position, just at the edge of the clump of bushes. The bird now began to hover above it, but still flew repeatedly to the porch roof and back to the original position of her nest. In 15 minutes she gained courage to alight upon the nest and cover the egg. Half an hour later she was driven off. She returned at once, following the edge of the bushes, and alighting upon the nest.

The nest was next moved 6 feet farther north. The bird returned along the edge of the bushes, hovered for a moment at the old nest site, then flew up and alighted upon the porch roof. From here she flew directly upon the nest, 5 minutes later she returned to the old nest site, from there flew out to sea, then back to the porch roof and to the nest.

On the following day the nest was moved 6 feet farther north. In the meantime the mate of the sitter had returned and both birds had become accustomed to the new position. When it was moved this time, instead of flying out to sea they both hovered about, striking at me, and they alighted upon the nest while I was still adjusting it. I moved it about for short distances in several directions, changed its level several times, and always the birds took it immediately. The visual stimulus of the nest seemed to have become the principal factor in orientation. To test this the stake supporting the nest was replaced by a tripod, the nest itself was covered by a black cloth, and the egg placed on top of this. (Such changes had been found to have no effect when made with nests otherwise undisturbed.) The sitter, the same one upon which former experiments were made, came back past the old nest site

directly towards the nest, swerved when 3 feet from it, and returned to the old nest site. This was repeated four times. She then alighted upon the nest, but flew away immediately. Her mate came up and both circled around the nest for some time, but did not alight. The cloth was removed and the first bird covered the nest within five seconds.

The nest was then moved 30 feet to the southward of its original position. The bird came back to the northern site, circled around for a moment, then alighted at the original position of the nest. She looked about, caught sight of the nest in its new position, flew directly to it, and covered the egg. A few minutes later she became dissatisfied and began to fly about, circling from the porch roof to the old nest site, to the nest, and back to the porch roof. She did not settle down before dark. On the following day both birds were observed at the nest, adding sticks to it and apparently quite satisfied. The next day both were gone and were not seen again.

These observations, like the preceding, lay emphasis upon the perch as a conspicuous visual stimulus from which the birds gain accurate orientation to the nest. When this method of reaching the nest failed, the birds came to depend upon the visual stimulus of the nest and were confused by the changes in the appearance of the nest.

The method by which the noddies recognize their nests is very similar to that of the sooties, the only difference being the manner of approaching the nest—in the one case by flying, in the other by walking. The perch has for the noddies much the same relation to the nest as the alighting place has for the sooties. Recognition of the noddy perch seems to be partly in visual terms, partly in the relation of the perch to other very conspicuous visual objects, such as the palm in experiment 10.

Destruction of both the alighting place and nest gave no very definite results. In one instance the birds flew back and forth along the edge of a row of bushes in which the nest and perch had been located, and made swift flights out around a number of tall stakes which had been planted to mark other nests. In almost every instance the destruction of the perch and removal of the nest brought out some antecedent factor in orientation, such as the stakes mentioned above, but these factors were so widely distributed and varied as to be practically beyond experimental control.

ORIENTATION IN APPROACHING THE ISLAND.

In the two experiments recorded it seems clear that the birds gained their first orientation from the visual stimulus afforded by the palm trees and buildings, and from a large number of observations I feel certain that all the birds, on approaching the island, determine their direction with respect to the more prominent visual features of buildings, trees, and bushes. The paths followed by the birds coming in from sea seem to give the best evidence of this.

Figure 2 shows the more prominent features of the island, the buildings and trees at the western extremity, a wide opening in the bushes above the southern beach, and the denser growth of bushes in the central and northern parts of the island. The prevailing winds during the nesting season were from the northeast. When the birds are frightened they run from their nests, leap into the air, and fly against the wind until they reach a height of from 10 to 25 feet, then turn quickly and fly with the wind until well away from the island. If not badly frightened, they turn when 20 to 200 yards in the lee of the island and fly back rapidly against the wind to the region of the nest. Birds in open areas

may be kept in sight constantly during the flight and time and time again are seen to follow the same path from and to the nest. The great majority of birds in such short flights return by rather definite pathways indicated by the dotted lines in figure 2. As will be seen from the distribution of the nests, these lines lead directly to the chief nesting areas, and the preponderance of birds following them may be due simply to this fact. In each case, however, the path is near a conspicuous landmark (the buildings and opening in the bushes), and the birds frequently change their direction of flight after passing these, as is indicated by line *d*, figure 2, which is the route followed by one bird in a number of trips to the nest. It seems almost certain that these prominent visual objects are the directing factors in the orientation of the birds when approaching the island.

SUMMARY OF WORK ON NEST RECOGNITION.

The general method by which the birds reach their nests may be summarized as follows: Coming in from the sea, they direct their flight by the more conspicuous features of the island, the buildings, prominent bushes, etc. From these the direction is taken along the shore-line, or the edge of the cleared nesting area to the alighting place, which also offers prominent visual stimuli. From this the path to the nest is followed, either by a series of visual-motor habits built up around other nests, débris, etc., or by a series of kinæsthetic-motor habits irrespective of external stimuli. Study of orientation at night is practically impossible, both because of the difficulty of identifying the birds and, chiefly, because of their increased timidity. During the breeding season, however, the nights are never so dark as to render the chief landmarks invisible at close range and, with direction gained from these, the birds' kinæsthetic-motor habits are adequate for further orientation.

The study of orientation in the nest locality has given no positive evidence upon the method of orientation at greater distances from the nest, but it does furnish two negative points of some importance. First, in the nest locality the birds are dependent upon visual and kinæsthetic habits for orientation and show no evidence whatever of the possession of any special sense of locality, such as a magnetic sense, functional within short flights. Second, kinæsthetic-motor habits are formed with no such rapidity as to suggest that the birds can retrace a path by memory of its successive directions and distances when these have been experienced only once. As in all animals, vision tends to be replaced by kinæsthesia as habits become fixed, but the process is relatively slow and plays no great part in orientation.

RECOGNITION OF THE YOUNG.

Closely related to the problem of orientation and recognition of the nest in the terns is that of the recognition of the young, and in this respect the two species show much greater differences than in nest recognition. The noddies do not distinguish their own young from other young noddies and I have been unable to discover any difference in their active behavior toward either strange noddy or sooty young.

EXPERIMENT 12.

I placed a sooty chick, 24 hours old, in the nest of a noddy, taking out the noddy egg which was just hatching. The noddy came back to the nest and covered the young sooty immediately. I watched the nest at frequent intervals throughout the day. The noddy brooded the young sooty constantly, but was not seen to feed it. On the second and third days the noddies continued on the nest, but were not seen to feed the sooty, which on the third day was considerably smaller than other chicks of the same age. On the fourth day, when the noddies were driven from the nest, the chick was found dead, apparently from starvation. Another sooty chick, 4 days old, was placed in the nest. The noddies continued to brood it for 3 days. On the fourth day the chick was dead and the noddies had abandoned the nest.

The noddies accept sooty chicks in exchange for their own young of any age, as was determined by a number of such substitutions. In no case, however, was a noddy seen to feed a young sooty. Apparently the peculiar behavior of the young noddy is required to call out from the adult the instinctive act of regurgitation and feeding.

EXPERIMENT 13.

Two 2-day old noddy chicks were placed in a nest which already contained a 3-day chick. One of the younger chicks was white, the other black. The older chick was black. The adult noddy returned and covered the three chicks immediately. The nest was observed regularly for 15 days, when the work was interrupted by heavy storms. The three young chicks were fed by the single pair of adults and throve as well as chicks which were alone in the other nests. On the eleventh day one of the chicks disappeared. The others were still in the nest when observations were interrupted.

During the first days after the hatching of the egg the noddies do not distinguish their own young from those of other birds and react to them only because of their presence in the nest. On the fourteenth day, or earlier, the young noddies leave the nest and lie hidden in the bushes during the day. In the evening they are fed, returning to the nest to meet their parents. Watson states that at this time the noddies distinguish their own young. I have no clear evidence upon this subject, but it seems probable that, as in the case of the sooties, the recognition is rather due to a dovetailing of the habits of parents and young than to a visual or auditory recognition.

Unlike the noddies, the sooties give prompt reaction both to the appearance of the chicks and to the number of chicks in the nest. They will not usually accept young noddies under any circumstances. In one instance a sooty which had been given a noddy egg hatched it and brooded the chick for a few hours in the morning, but in the evening both adult and chick were gone. Usually where a young noddy is placed in a sooty nest the old bird throws it out and drives it far away into the bushes. The reaction is to the visual stimulus,* and is purely instinctive, as is shown by such results as the following:

EXPERIMENT 14.

(*a*) I interchanged a sooty egg, just ready to hatch, and a 24-hour chick. The parent of the egg accepted the chick immediately. The other hesitated in covering the egg, looked about, walked to and fro for several minutes, and finally settled down upon the nest.

*Figures of young noddies and sooties are given by Watson, *opus cit.*

(*b*) I replaced a pipped sooty egg with a young noddy. The sooties attacked the young noddy and drove it from the nest. A third sooty from another nest came and helped in the attack and I was forced to interfere to save the chick's life.

Such reactions are by no means invariable. A sooty will sometimes refuse to accept a sooty chick in exchange for its egg or again will fail for some minutes to notice the presence of a young noddy in its nest; but in general the different appearance of the chicks of the two species calls out different instinctive behavior in the adult sooties.

The greatest variation in the behavior of the adult sooties occurs in their reaction to strange chicks of their own species. Ordinarily they do not distinguish between their own young and other sooty chicks of about the same age during the first few days after hatching.

EXPERIMENT 15.

I replaced a 3-day chick in a nest with another of the same age. The parent returned to the ground near the nest, rushed forward, and threw out the nearest chick (her own), drove him into the bushes, then walked back and covered the other. I placed the abandoned chick in a nest with another of about the same age. One of the parents of this chick returned and accepted both young. Three days later both chicks were still in the nest.

Chicks from 1 to 4 days of age may almost always be interchanged without inducing any marked change in the behavior of their parents. Many such exchanges were made and almost without exception the changelings were adopted without hesitation. Until the age of 4 days the sooty chicks are not recognized individually by their parents. At about that age a change takes place in the behavior of the young sooty. Before this time it feigns death when attacked by adult birds or when removed from the nest to strange surroundings. On the fourth or fifth day it becomes much more active, attempts to crawl out of the nest and hide in the bushes at the experimenter's approach, and attempts to run away when attacked in the open by older sooties. After the age of 10 days it spends most of the daylight hours lying hidden among the weeds and bushes near the nest and comes out only in the evening to receive food from the parents. With its increasing activity it seems to form a set of habits with respect to its nest very similar to those of the adult birds. In its own nest it lies quiet when attacked by strange adults, which happens frequently, for the sooty rarely misses an opportunity to take a sly dig at her neighbor's chick; but outside its own nest the chick, after the fourth day, scrambles away actively when attacked. This seems to furnish the first criterion by which the adults recognize their own young.

EXPERIMENT 16.

(*a*) I interchanged a 1-day and a 6-day chick. The parents at both nests were much disturbed, refused to cover their nests, and poked the young ones with their beaks. The younger chick lay still in the nest and after a short time seemed to have been adopted. The older chick started to run away and was immediately attacked savagely and driven away among the bushes.

(*b*) I exchanged a 3-day and a 5-day chick. The younger bird was accepted immediately by the parents of the other. They did not seem to note the change. When the adults returned to the other nest the 5-day chick ran away. They attacked it at once and drove it away to a distance of 4 feet, where it received so much abuse that it gave up and refused to run further, feigning death. I returned it to the nest. The adults examined it and pushed it away roughly. This time it lay quite motionless, however, and the older birds, after driving away a trespasser, adopted it.

The tendency of these chicks to run away from the strange nests no doubt determined the parents' final action toward them. In the first experiment recorded here, the difference in the appearance of the chicks, differing in age by 5 days, was sufficient to cause some disturbance in the behavior of the parents, but was not great enough alone to cause the rejection of the chick.

On the seventh or eighth day after the eggs hatch, the birds begin to distinguish their own chicks from younger or older ones by their appearance alone.

EXPERIMENT 17.

(*a*) A 2-day chick was placed in a nest instead of the 7-day one which belonged there. The adults pulled it out of the nest and forced it to crawl away. It was immediately adopted by a bird whose nest had been robbed of a young chick upon the previous day.

(*b*) A 1-day chick was placed in a nest instead of an 8-day chick. The parent of the 8-day chick returned and refused to cover the nest for 10 minutes. She continually pecked at the chick, although not savagely. When she showed a tendency to leave the nest, I replaced the young bird with one 3 days old. The parent returned, pecked the chick, and when it started to run away, attacked it furiously. Her own chick was replaced and accepted at once. After a time I substituted a 10-day chick for this one. It was attacked and driven away at once.

The parent sooty here seemed to distinguish its own young, 8 days old, from 1-, 3-, and 10-day chicks which were substituted for it. But the recognition at this time is by no means certain, as is shown in the following observations:

EXPERIMENT 18.

A 6-day old chick and a young egg were interchanged. The parent of the egg drove the chick away immediately. The nest from which the chick was taken was under a clump of bushes. The owner of the nest examined the egg, came out from under the bushes, went back and covered the egg, then came out again. She began to show an interest in the cries of her chick, which was being abused by the egg parent about 3 feet away. She went over and defended the chick, and finally covered it, about 4 feet from her own nest.

I next substituted a 5-day sooty for the egg and replaced the 6-day chick in the egg nest. Its parent returned and found the egg parent attacking it. She rushed to its defense and covered it near the egg nest. Soon she caught sight of the young bird in her own nest. She went over to it and, after looking at it carefully, drove it out of the nest. I replaced it. She returned and accepted it.

Her own chick was put back in the egg nest. She returned, defended it and covered it, about 2 feet from her nest, paying no attention to the strange chick in her own nest. The chicks were then interchanged, the 5-day one being placed in the egg nest and the 6-day one in its own nest under the bushes. The old bird returned and defended the 5-day chick stoutly against the attacks of the egg parent. She followed it for several feet as it ran away and finally covered it. Her own chick caught sight of her and began to squall. She was very much disturbed, got up and started toward it, turned back, and after much hesitation finally went to her own chick in answer to its vigorous cries. As the 5-day chick was always silent this may have furnished the basis for discrimination.

I placed a chick a very little larger than the 5-day chick in the nest under the bushes and the 5-day chick in the egg nest again. The parent started to defend her own chick in a half-hearted manner, caught sight of the chick in her nest, went to it and covered it. A moment later she drove it out of the nest and stood for 5 minutes in the empty nest. I put both chicks in her nest. She pecked the larger, but as it did not run away she covered both.

Half an hour later both chicks were still in the nest. I placed the strange one in the egg nest, where it was promptly attacked. Its foster-mother paid no attention to it, but went to her own chick and nest. I interchanged the chicks. She rushed to defend her chick, covered it, and paid no further attention to her own nest.

The reactions of this adult bird are rather conflicting and hard to interpret, as is true, indeed, of much of the behavior of the birds in tests upon recognition of the young. Seemingly, there is here the beginning of recognition of the

chick, based in part upon its appearance, in part upon its "feel" in the nest, its cries, and its reactions to the parent. None of these alone seems sufficient to form the basis of an absolutely positive recognition, and it is probable that the conflicting behavior of the bird is due to the imperfect integration of some such groups of impulses as the following:

(1) Positive reaction to the appearance of the nest and its contents, and to the nest locality.

(2) Disturbance of this caused by the changed appearance of the young and leading to greater sensitivity to other stimuli.

(3) Postive reaction to feel of chick in the nest; this likewise disturbed by the substitution of the larger chick.

(4) Positive reactions to the cries of the chick.

(5) Defending reaction to the situation, *chick attacked by adult.* (For more detailed examples of this see p. 83).

(6) This defending reaction is established as a habit which temporarily exceeds even (1) in force.

All these factors are apparent in the activities of this bird and stand out more or less clearly in the reactions of most of the sooties to their own and to other young. Recognition of the young at this stage thus appears to be the result, in part, of the pattern of visual and tactual stimuli offered by the chick; in part, of the behavior of the chick in the nest region (experiment 16). Complete recognition is the result of a complex of many sensory-motor reactions, not merely of a single type of stimulus.

With older chicks, after the tenth day, it is almost impossible to carry out observations unless full time may be devoted to them. They lie hidden among the bushes during the greater part of the day and come out to their nests only at the return of their parents. They are extremely wild and run to cover whenever the colony is disturbed. They show a familiarity with the nest region almost equal to that of the adult birds, returning readily to the nest alone, threading their way among other nests and avoiding the attacks of strong adults. From such observations as I have been able to make I believe that the chicks usually take the same cover and that both chick and parent become perfectly familiar with the details of the most direct path from this to the nest, meeting one another at various points along the path and reacting to one another, at least in part, by virtue of their presence in this familiar neighborhood.

Watson has shown that when the appearance of adult birds is completely altered, as by painting their breasts and head with brilliant colors, they are at first rejected by their mates, but this rejection is not permanent and the disfigured birds are finally accepted by their mates by reason of their persistence in their normal activities in the region of their nests. The "recognition" through the interrelation of habits overcomes the disturbance produced by the changed visual situation. For the recognition of the young, the same complex of reactions must be established. The visual stimulus provided by the young at the nest calls out the normal brooding activities. A slight change in the appearance of the young interrupts the normal series of activities and leads to trial movements. If the situation remains otherwise unchanged—if the behavior of the young gives the customary series of stimuli—the changed appearance comes to be disregarded.

INSTINCT AND HABIT IN THE NESTING ACTIVITIES.

MODIFIABILITY OF INSTINCTIVE ACTIVITIES.

The activities of the birds centering around the nest during the breeding season may be roughly divided into mating, selection of the nest site, construction of the nest, laying, incubation, and the rearing of the young. During each of these periods a special group of instinctive acts is called into play and the behavior of the birds is modified quite suddenly at the transition from one period to the next, as in the sudden appearance of the constant brooding activities after the egg is laid. Watson found that birds which are quite timid become aggressive as soon as the egg is laid and either refuse to be driven from the nest or attack intruders actively. He was able to induce an early appearance of this change in behavior by placing an egg in a newly completed nest, and thus raised the question of the relation of these changes in instinctive behavior to external stimuli and to internal processes.

I have obtained a small amount of evidence bearing upon this question, but the data are fragmentary and a great deal more time may be spent profitably upon this particular aspect of behavior. Owing to the lateness of the season when the work was begun no observations were obtained upon mating or the construction of the nest.

EXPERIMENT 19.

After some difficulty a newly completed noddy nest was found. The building activities of the owners had almost ceased, but no egg had been laid and the birds were not occupying the nest regularly. At the first observation, one of the birds was perched on a dead limb near the nest and the other was not in the neighborhood. A noddy egg was placed in the nest. This was early in the morning. No change in the activity of the bird at the nest was observed during the day. It disappeared occasionally, but did not show any reaction to the egg. The mate was not seen.

On the following morning a bird was found brooding the egg. Its mate was on the dead limb near by. When I approached both birds defended the nest, the one on the perch flying into the air and striking at me, the sitter refusing to leave the nest. I caught the latter and marked it with a spot of paint. The marked bird was occupying the nest when it was observed later in the day.

On the third day only the unmarked bird was seen at the nest. It spent the day sitting on the perch or on the edge of the nest, paying no attention to the egg.

On the fourth day the marked bird was again observed at the nest. Both birds were upon the nest, the marked one brooding the egg, the other standing on the edge of the nest. Both flew up at my approach. The marked bird returned immediately and covered the egg. The unmarked one returned, hovered about for a short time without alighting, and then flew out to sea.

On the following day the marked bird was observed on the perch. At this time the unmarked bird was on the nest over the egg. For as long as I watched, this bird did not assume the brooding position, but stood upright and turned about, moved to the edge of the nest, and flew away rapidly when disturbed, thus showing the behavior of the layer rather than of the sitter.

The marked bird was on the nest for the greater part of the sixth day. The unmarked bird was seen on the edge of the nest, but did not cover the egg.

On the seventh day a second egg had been deposited and the unmarked bird spent the day in brooding the eggs. After this time the birds seemed to take turns regularly in brooding the eggs.

In this case it seems clear that only one of the birds was stimulated to the brooding phase of activity by the sight of the egg. The other retained almost wholly the behavior of the layer up to the time when the second egg was depos-

ited. There can be little doubt that this second bird was the female, that the stimulus in the case of the male was purely visual and tactual, while in the case of the female the act of laying was necessary before the brooding phase could appear.

It is generally stated by the natives of the region that the sooty will lay a second egg if the first is taken from the nest. I was unable to verify this. The eggs were taken from 10 nests and in no case was a second egg deposited; the nests were all abandoned after a few days. However, none of these nests was less than a week old, and it is possible that the loss of the egg at an early period may serve as a stimulus to further oviposition.

An attempt was made to find out how long incubation might be interrupted by removal of the egg without leading to the abandonment of the nest. Eggs were taken from a number of nests and returned at regular intervals, from 6 hours to 4 days. Brooding activities were resumed by the birds after 4 days. The experiment had to be interrupted here because of stormy weather.* From rough observations, however, I believe that after a week's interruption the birds will not resume incubation. The time is certainly less than the time required for the loss of the brooding instinct of birds in captivity,† as might be expected from the greater opportunity which free birds have to form new habits toward the colony.

After the hatching of the egg the parental instinct seems to be even more dependent upon the stimulus of the chick in the nest. In some cases nests from which the chicks were removed seemed to be wholly abandoned after 48 hours.

The second marked change in the behavior of the nesting birds occurs at the hatching of the eggs. The incubation period of the noddies is given by Watson as 32 to 35 days, that of the sooties as 26 days. I made attempts to shorten and to lengthen the time required for incubation in order to test the extent to which intra-organic factors are concerned in the changes from incubation to brooding activities. The experiments are very unsatisfactory, chiefly because the age of very few nests was known.

Substitution of young chicks for noddy eggs of any age was not followed by any marked change in the behavior of the adult birds. They were never seen to attack the chicks, but I could not determine how soon such chicks are fed. It seems certain, however, that the incubation period of the noddy may be shortened by at least one-fourth without serious interference from an intra-organic rhythm.

Attempts to shorten the incubation time of the sooties have not been carried out fully, but there is more evidence that the length of the incubation period is determined, to some extent, by internal factors. Parents whose eggs are less than two weeks old will not adopt young chicks. The data at hand are not conclusive beyond this point. Adults whose eggs are pipped will sometimes refuse to accept young birds in exchange for them, but usually during the last week of incubation such changes may be made without difficulty.

*Experimental work with the colony was made almost impossible during the last 10 days of June, at a time when the young birds were in the most interesting stage of development. High winds and heavy rains, with a considerable lowering of the temperature, made it advisable to disturb the colony as little as possible, as the birds frightened up from the ground were instantly blown far out to sea and at times could not regain the island for hours.

†See page 43 of this volume.

The period of incubation may be lengthened also to some extent, but this differs in different birds and its limit was not determined.

In 10 nests younger eggs were substituted for pipped ones. After 2 days one of the nests was abandoned. During the following 5 days 6 of the eggs hatched. The remaining birds were still sitting at the end of 7 days, when observations had to be discontinued.

A dead egg was substituted for a pipped one, somewhat earlier in the season. Upon the following day it was found that the parents had adopted a 3-day old chick and were brooding both it and the egg.

This observation and the abandonment of the single nest in the other experiment suggest that the length of the incubation period is conditioned by an internal rhythm, which does not determine, however, an exact period of 26 days, but permits a range of variation of 2 weeks or more around this period.

INTEGRATION OF COMPLEX HABITS.

Two of the most striking features in the colonial life and behavior of the terns are the enormous number of habits which they exhibit and the disconnected, impulsive character of their activities. The number of habits involved in the process of orientation has been illustrated in the earlier experiments. By the time that the experimental work had been carried far enough to give an insight into the mechanism of orientation the eggs were all laid and the birds had been reacting to the situation at the nest for some weeks. Hence there was little opportunity for study of the formation of the habit complexes. In general, as might be expected in a colonial bird, the most definite and firmly established reactions are associated with other birds or their nests. The chief landmarks used by many sooties for the final location of their nests are the nests of their neighbors, and it is apparent that the presence of these neighbors, defending their nest areas, tends to give a greater stimulating value to the nests. The birds whose nests were in very crowded areas showed a much lower plasticity in adapting to changed conditions than those whose nests were somewhat isolated, hesitating to occupy nests only a few inches from their own nest sites and offering little defense of such nests against trespassers. The behavior of the birds suggests that in the early part of the nesting season exchanges of nests must be of relatively frequent occurrence. An interesting example of the diffidence of the birds in appropriating new nests is given in the following:

> Two sooty nests, 18 inches apart, were obliterated and both eggs were placed in a new nest half way between them. The parents of both eggs returned to their nest sites, missed the eggs, and began to look about for them. One caught sight of the eggs in the new nest and covered them. The other at once drove her off and took the nest. The first, after standing for a few moments at her nest, returned to the new nest and drove off the second. This was repeated several times, the bird occupying the nest offering little resistance to the attacks of the other but returning fiercely to the attack after a visit to her own empty nest. Finally one bird rolled an egg back to its nest and both settled down contentedly.

In small matters, such as changes of a few inches in the position of the nest, changes in the appearance of the egg, young, or nest, the habits of the birds may be quite readily modified, but this seems to hold true only within certain limits. Many attempts were made to shift nests to the edge of the beach with the purpose of transferring them to rafts in the hope of ultimately transplanting a part of the colony to other keys, but however slowly such shifts

were made (6 inches or so per day) the nests were either abandoned when moved 4 feet or less from their original position, or the eggs were rolled back as fast as they could be moved forward.

NOTE.—This moving of the egg back to the original position of the nest has been mentioned before in connection with other experiments. It might seem to involve very complex processes, memory of the relative position of the egg and nest and an attempt to restore their relation. I watched the entire procedure in a dozen or more instances and believe that there is little implicit behavior involved in it. The bird, returning to the empty nest, misses the egg, looks about, and catches sight of it a few inches away. She starts towards it rapidly, but goes more and more slowly as she approaches, sometimes turning back before reaching it. Perhaps after several such hesitating starts she gets close to the egg, stops as soon as she can reach it with her beak, and rolls it back under her body. In this way the egg is moved for 2 or 3 inches at each trip from the old nest site to the egg. That it is moved in the direction of the old nest site seems to be the result of the fact that the bird always faces away from the latter as she approaches the egg. There is no real evidence of implicit behavior in this case, even of a delayed reaction in which orientation is held as the egg is moved.

In all its activities, where reaction to a new situation is involved, the behavior of the birds has a peculiarly impulsive character. One group of stimuli seems to gain momentary control and determine the bird's reaction in spite of contradictory elements in the situation as a whole. If a chick is taken from its nest and put down among strange adults they are at once attracted by its cries and crowd around it. Several of them attack it and immediately others rush forward to defend it. The fight becomes general and may give the chick a chance to escape. After a time some of the adults are driven away and leave the victors to strut about. If one of these catches sight of the chick, he attacks it and the fight is soon renewed. I have seen the same bird alternately attack and defend a chick through a number of fights; the sight of the strange chick calls out movements of attack; the sight of an adult attacking a chick calls for defense of the chick. Young chicks, when attacked by adults, either feign death or sprawl about helplessly, without being able to escape. In the latter case they sometimes force their way under the body of the adult. When this happens it practically always produces a marked change in the behavior of the adult bird. His aggressive attitude drops away suddenly, he shuffles about for an instant, looks down at the chick, tucks it under him with his beak, and assumes the brooding position. If he is at some distance from his nest this brooding reaction is of short duration; he soon grows restless, gets up and turns around several times, and finally returns and attacks the chick with renewed savagery.

The same lack of coördination in reactions to complex situations is seen in the choice between two nests, in the re-orientation in the path to the nest, and in the reaction to changes in the appearance of the path. The reactions are to separate groups of stimuli and there is clearly no analysis of the situation as a whole. The birds do what a man would only consider doing under like circumstances; that is, they carry out in overt activity many of the same processes which in man are restricted to the language mechanism. In subjective terms, they show little or no evidence of "ideational processes" in their activities.

STUDIES ON THE SPECTRAL SENSITIVITY OF BIRDS.

By J. B. WATSON.

STUDIES ON THE SPECTRAL SENSITIVITY OF BIRDS.

METHODS OF STUDYING SPECTRAL SENSITIVITY.

In the literature bearing upon the homing sense (distant orientation) one meets with the theory* that possibly those animals in which such a function resides possess retinas sensitive to extremely long wave-lengths. It is further supposed by the adherents of such a view that these long waves follow the surface of the earth.† As a consequence, any animal with very keen vision, having a retina sensitive to them, could directly respond to the visual stimulus of a distant goal (côte, nest, etc.), the curvature of the earth by hypothesis not interfering with the continuity of vision. Such a theory, while speculative in the extreme and without good physical support, can not be dismissed without some experimental tests, especially in view of the fact that no hypothesis of return (logically) simpler than this has yet been advanced.

During the spring of 1912 we decided to make a careful test of the limits of spectral sensitivity in the noddy tern and in the sooty tern. It is clear that the above theory has no standing if it can be shown that the animal under consideration has a spectrum coextensive at the red end with that of man ($\lambda = 7600$). In order to make the work more complete we purchased some chicks (of mixed breed) for testing in the same apparatus. We set up the apparatus, consisting of a large spectrometer with suitable compartments for controlling the responses of the birds, in the Marine Biological Laboratory of the Carnegie Institution of Washington, Tortugas, Florida. After capturing and rearing 12 young noddies and 12 young sooties and constructing a rather elaborate dark room, we found that the small electric-light plant which we had installed could not be run with sufficient steadiness for use with the Nernst lamp. We finally decided to abandon the experiment at Tortugas and to remove the apparatus and the birds to the Psychological Laboratory of The Johns Hopkins University.

The birds arrived safely, standing the long journey by rail very well. They were placed in a large yard and fed twice daily with chopped fish. For some reason they did not thrive, and died one by one. By the time the apparatus could again be made ready all the terns had died. There remained only the three chicks brought from Tortugas. We thought it distinctly worth while to test them, since they are day birds and especially sensitive to red rays, as has been shown by Hess.‡

*Faculté de direction du pigeon voyageur. Ann. de Psych. Zool., I, 1901, p. 22; II, 1902, p. 48. The above article was written by Hachet-Souplet. Duchâtel's view appears in the article in a series of letters.

†Duchâtel and Hachet-Souplet, as has been pointed out by E. Branley, in the article referred to above (II, p. 57), are apparently not familiar with the properties of light. Duchâtel at least draws an analogy between the Hertzian waves and the infra-red rays. Now the Hertzian waves do apparently follow the surface of the earth (over water at any rate; as to their course over a mountainous region there seems to be some question). But the Hertzian waves have a length varying from a few centimeters to many meters, and are not to be compared with the relatively extremely short infra-red rays which may be obtained from prismatic spectra. Contrary to Duchâtel, the violet rays would come nearer fitting his theory, since they suffer greater refraction by the different layers of the earth's atmosphere. Had his theory been based upon ultra-violet rays rather than the infra-red, it would have had slightly better physical support.

‡Hess, C., Ueber Dunkeladaptation bei Hühnern und Tauben. Archiv f. Augenheilkunde. 57, 298; also 57, 317.

The experiment naturally falls into two parts: first, accurate determination of the limits of the spectrum at the violet and red ends; second, plotting of the sensitivity curve for monochromatic light. At the end the attempt is made to compare these findings with those obtained from similar tests upon man.

A very large spectrometer was used, the ground plan of which is shown in figure 8. It consists of the source L, a Nernst automatic lamp burnt at 110 volts, D. C. The lamp was controlled by regulating the amperage (the ammeter reading to $\frac{1}{10}$ of an ampere); a single achromat condenser gave an image of the source upon the slit S (this slit is accurately controlled by a micrometer screw); the collimator Cm, which is a compound 4-inch portrait lens; the dense flint-glass prism P, with 4-inch face compound portrait lens O, used as the objective, and the slit S_2, which was mounted upon a carriage controlled by a micrometer screw. The slit system was directly calibrated in terms of wave-length, so that any possible region could be immediately admitted to the dark room.

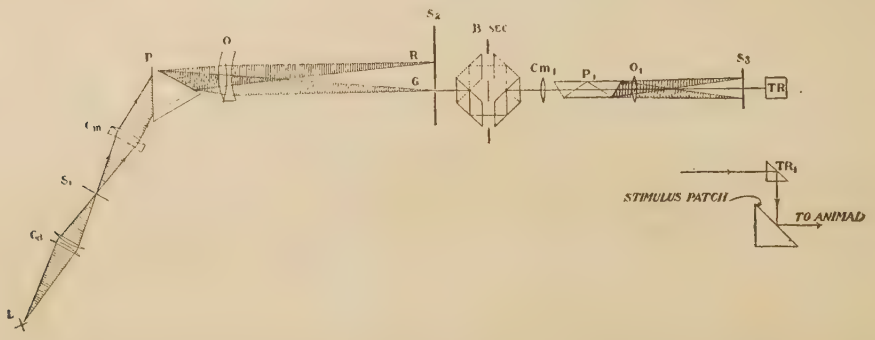


FIG. 8.—Ground Plan of Double Spectrometer. L, source (Nernst); Cd., condenser; S_1, slit; Cm., collimator; P, dense flint glass prism; O, objective; S_2, selecting slit; B. sec., Brodhun rotating sector; $Cm._1$, collimator for auxiliary spectrometer; P_1, direct vision prism; O_1, objective for auxiliary spectrometer; S_3, final selecting slit; TR., total reflection prism throwing light on plaster surface. The small sketch at the right is a vertical section through T. R., showing the way the beam is finally projected upon the plaster surface (stimulus patch).

For the work on limits the Brodhun sector (B. sec.) and the auxiliary spectrometer shown in the diagram were not used. These were employed in the second part of the work—the determination of the relative stimulating effect of the different spectral regions.

The Brodhun sector is too well known to require detailed description. It consists of a series of rotating prisms which revolves the light around a stationary sector. It permits of the immediate control of the intensity of the light without stopping the motor for the needed adjustment. The sector openings are controlled by an arm extending from the housing of the sector. An arbitrary scale in millimeters running from zero, where the sector is fully closed, to 140, where the sector is open at the maximum allowed by its construction, permits one to adjust the intensity of the light with great accuracy.

For reasons which will appear in the text, it was found necessary, in the second part of the work, to secure monochromatic light practically free from admixture with white light. This can not be secured with a simple spectrometer system. Accordingly, after a given band of light had been selected by the slit S_2, it was passed through a second spectrometer, of which Cm_1, in

the drawing, is the collimator, P_1 the direct vision prism, O_1 the objective, and S_3 the final slit. The total reflection prism TR throws the beam vertically down on the plaster-paris surface (stimulus patch 3 cm. × 5 cm.), which is placed in the compartment in which the response of the animal is obtained. Issuing from this second spectrometer system is a band of monochromatic light, which, when examined with a pocket spectrometer, shows no further resolution.

In determining the curve of the relative stimulating value of light of different wave-lengths the bands of monochromatic light were equated in energy. Since thresholds were involved, the calibrated selenium cell, by means of which the energy was determined, was put 10 cm. above the stimulus patch and directly in the path of the beam. A. H. Pfund's method of calibrating with this cell will be found in Yerkes and Watson.* Even calibration before diffusion did not give a beam sufficiently low in intensity for direct use in threshold work. It was found necessary to introduce a wedge of smoked glass behind the final selecting slit. In the yellow-green, green, and blue regions a very dark glass was required; in the yellow and orange regions one very much

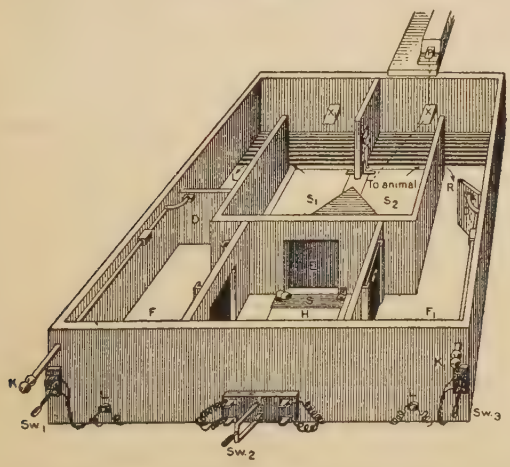


FIG. 9.—Control Box. H, home box; C, response chamber; F, F_1, food compartments; X, X_1, stimulus patches; S_1, S_2, platforms causing lights L, L, to wink; S, platform causing both lights to wink simultaneously; D, D_1, doors leading to food compartments; K, K_1, knobs controlling these doors.

lighter was required. Finally, in the red region no glass at all was required. Since these glasses were inserted after calibration, it was necessary to send them to the Bureau of Standards for calibration as to absolute absorption at the different wave-lengths employed in this work. The curve (fig. 10, p. 98) contains the proper corrections. When the appropriate smoked glass was inserted the threshold for the animal was found to lie well within the range of intensities offered by the Brodhun sector.

The animal's reactions were taken in the apparatus shown in figure 9, which consists of a large box, 94.5 cm. in length by 74 cm. in width by 25 cm. in depth. It is divided into a home compartment H, 31.5 cm. in length by 25 cm. in width, and a response chamber C, 30.5 cm. in width, which is partially separated into two smaller compartments by a partition. The length of the partition is 25.5 cm. The distance from the door leading into the response chamber to the stimulus patch is 69 cm. The distance between the stimulus patches is 25 cm. The dimensions of the face of the stimulus patch are 3 cm. by 5 cm. The other divisions of the box are not important.

*Methods of Studying Vision in Animals, Behav. Mono., 2.

It can readily be seen that the box offers two alleys which lead into food compartments, F and F_1. Two doors, D and D_1, permit one to confine the animal in the proper food compartment. By a special mechanism the doors can be closed after the passage of the animal from without the apparatus by pulling knobs K and K_1. Two doors lead from F and F_1 into H. It is thus possible to work with the animal without touching it and without being seen by it.

By means of a special signaling device the movements of the animal can be recorded without the necessity of watching it. Two miniature lamps, blackened except for an opening about 1 mm. in diameter, are electrically connected with the platforms S and S_1 and S_2. When the animal crosses S and goes into the response chamber both lamps are caused to wink (break circuit). If he then crosses the right-hand platform S_2, the right lamp goes out until the reaction is completed and the animal turns into R. If the animal goes across S_1, the left lamp goes out, etc. In the drawing the stimulus light falls upon the right stimulus patch X; consequently food may be obtained by passing around R through the open door D_1. It will be noted that the door D is closed. If the animal makes the wrong choice it must pass S_1, around into alley L; there finding the door closed, it must retrace its steps and pass S_2, into R through D. The apparatus as a whole can be shifted so that the light may be made to fall either upon X or X_1 at will. Although arranged for punishment by electric shock, punishment was not used in the experiments upon the chicks.

THE LIMITS OF THE CHICK'S SPECTRAL SENSITIVITY.

The experiments were carried out upon three chicks. Experimentation began on August 30, 1912. On that date the chicks were exactly 30 days of age. Before actual tests were made the animals were forced to go through the complete reaction they would have to use during regular experimentation; that is, they were at first confined in the home box, then admitted to the reaction chamber, and then forced to go to the right or left food compartments at the will of the experimenter. These preparatory exercises were conducted in a well-lighted room. One of the interesting things appearing in these tests with both chicks and terns was the failure of the birds to learn readily to pass around into the alleys L and R. Some rats which the writer tested in the same way learned to make these turns in a few trials. It took nearly 3 weeks to get the birds to run rapidly from entrance into the stimulus chamber around to the food. When they had learned to make the turns, regular tests were begun. These consisted in responding positively to the compartment illuminated by the monochromatic light (or negatively to the dark compartment).

The chicks were brought into the dark room in a basket and allowed to remain in complete darkness for 10 minutes. Then a 16 c. p. tantalum light placed 1 meter above the center of the home box was turned on and a chick was placed in H. The chicks are designated by D, R, and G. D was always experimented upon first; then in order, R and G. The state of adaptation for the three chicks was thus not quite the same.* As soon as a chick was placed

*And yet no marked differences in sensitivity of a progressive kind were noted. The use of the white light after every response, as above described, probably kept adaptation constant in all. The two chicks not undergoing experiments were kept in the room with the apparatus. The wicker basket in which they were confined admitted light with readiness.

in H the tantalum light was turned out. Five seconds later (by metronome) the door to the reaction chamber was opened. The chick then passed out and reacted either positively or negatively to the light. When the animal had immediately, or after errors, finally correctly responded (*i. e.* positively), as shown by causing the proper light to wink (p. 90), the tantalum light was again turned up. After feeding for 3 seconds the animals were allowed to pass again into H through one of the side doors. The light was again turned out for 5 seconds and the test repeated. At first only 6 to 8 tests were made. The number was then increased to 10 and sometimes in control work to 15 to 20. In the following tests no effort was made to control the energy of the stimulus. The light falling on the plaster surface was very intense—the maximum afforded by the spectrometer at any given wave-length. We planned to test the animals first near the center of the spectrum, $\lambda = 5480$, and then to pass on in the red region until a limit to response was reached; next to retrain on $\lambda = 5480$, and then to pass to the violet region, continually decreasing the wave-length until the limit was reached. Table 23 gives a summary of the tests at $\lambda = 5480$, showing that the retina of the chick is sensitive to $\lambda = 5480$, and furthermore, that the chick is *distinctly positive* to light.

TABLE 23.

No. of trials.	Animal.	Per cent right.	Remarks.
29	D	93	Two errors were made on first day.
29	R	90	Only one error after first day.
29	G	95	

LIMITS AT THE RED END.

In order that no useless work might be done in testing the chicks at intermediate points, I next tested them in a similar way at $\lambda = 6230$; then at $\lambda = 6520$ and $\lambda = 6825$ (table 24).

TABLE 24.

	No. of trials.	Animal.	Per cent correct.	Remarks.
$\lambda = 6230$	12	D	100	
	12	R	100	
	9	G	100	
$\lambda = 6520$	6	D	83	
	6	R	100	
	6	G	100	
$\lambda = 6825$	8	D	87	Animal badly disturbed, very slow, cried a great deal.
	8	R	87	Considerably slowed.
	5	G	100	After fifth refused to work. Turned slit back to $\lambda = 6520$; animal worked well at this point.

As the animals were somewhat disturbed on the final test, due probably to the low stimulating value of the light, I increased the intensity by opening up the slit in the collimator S_1, with the results shown in table 25. The

time of the response was also recorded.* The tests shown in table 25 were made on successive days.

Since the animals were holding to such a high percentage of correct choices, I became suspicious as regards the stimulus to which they were responding. I convinced myself speedily that there were no secondary criteria, such as noise, rotating of the box, etc., which could be responsible for their reactions. The animal would not respond in any definite way when the monochromatic light was excluded. It became evident that the illumination on the plaster surface was responsible for their actions. I next turned the slit far out beyond the limits of the human spectrum ($\lambda = 8000$) and found that the animals still reacted correctly. Under the conditions of this latter test the plaster surface was invisible to me, but after darkness adaptation it became faintly visible. This was undoubtedly due to the stray white light present in this simple spectrometer. I had not supposed its intensity great enough to create disturbance in animal work of this kind. Such a source of error is not mentioned in the voluminous works on color responses in animals.

This brought me into consultation with my colleague, Professor A. H. Pfund, who assured me that there was always a large amount of such light in both prism and grating spectra, and that there were two methods of eliminating it: one by putting behind the slit in the objective a filter of such a nature that there would be transmitted only the region of the spectrum desired. In this way all diffuse white light could be eliminated. The other method was to use the purified spectrum of Helmholtz, *i. e.*, to take the monochromatic band as it issues from the first spectrometer and to pass it through another spectrometer. All diffuse white light in the beam so chosen is thrown off into a secondary spectrum. Mention (p. 88) has already been made of the set up of this auxiliary spectrometer.

TABLE 25.

	No. of trials.	Animal.	Per cent right.	
$\lambda = 6825$	6	D	100	*
	6	R	100	
	6	G	100	
$\lambda = 7000$	6	D	83	
	6	R	100	
	6	G	100	
$\lambda = 7195$	6	D	83	
	6	R	100	
	6	G	100	

*Evidently increased intensity made response easier.

The first method is not satisfactory for use in experiments where it is necessary to pass rapidly from one region of the spectrum to another. The second method is the only thoroughly satisfactory one in such work as the determination of the relative stimulating value of waves of different length.

Since the work on limits required only two filters—one at the red end and one at the violet—it was at the time decided to be more convenient to use them than to wait for the construction of the double spectrometer. By the time the work on limits had been completed the second spectrometer was ready for use. It is certainly advisable, when the apparatus is at hand, to employ the "purified spectrum," even in the work on limits.

*The time of all responses was taken from this point on throughout the remainder of the experiment. It is without general significance, so is not given. It aided the experimenter materially in keeping adaptation reasonably constant. As a matter of fact, as the limit is approached, the time increases enormously. The chicks attempted to form the habit of waiting in H until adaptation brought the light over the threshold. This could be checked by waiting until the normal time for reaction had passed, then closing the door, turning up the light for 5 seconds, then turning it out and repeating the regular routine of the test. The time of normal reaction varied from about 2 to 14 seconds.

Pfund has found two filters which work well in the determination of limits. The filter for the red end consists of an aqueous solution of cyanin and potassium bichloride placed in a convenient glass cell with plane parallel sides. Such a cell was placed just behind the selecting unit, which was set at $\lambda = 7000$. This filter transmits 80 per cent of the light from $\lambda = 7000$ up to 1μ, but excludes all light below $\lambda = 7000$. When placed behind the slit, the red surface was clearly visible to me after 20 seconds of darkness adaptation.* The slit in the collimater was set at 0.75 mm. and that in the objective at 0.52 mm. The complete records of the chicks are shown in table 26.

On the following day I set the slit at $\lambda = 7150$ with other conditions exactly as before. The light was quite faint to me. After 5 minutes' adaptation to darkness the red surged in quite clearly.

TABLE 26.

$\lambda = 7000$.	No. of trials.	Position of light.	Response.		Remarks.
Chick D	1	Left	Left	62 per cent	I waited before admitting animal until light was visible to me.
	2	Left	Right		
	3	Right	Right		
	4	Right	Right		
	5	Left	Left		
	6	Right	Right		
	7	Left	Right		
	8	Right	Left		
Chick R	1	Right	Right	88 per cent	I waited before admitting animal until light was visible to me.
	2	Right	Right		
	3	Left	Left		
	4	Left	Left		
	5	Right	Right		
	6	Right	Left		
	7	Left	Left		
	8	Right	Right		
Chick G	1	Right	Right	100 per cent	I allowed this animal to make choice 10 seconds after turning out tantalum light, regardless of whether I could see it. It was below my threshold in nearly all cases when he made his choice.
	2	Right	Right		
	3	Left	Left		
	4	Left	Left		
	5	Right	Right		
	6	Right	Right		
	7	Left	Left		
	8	Left	Left		

Chick D chose correctly 3 times, then made 3 consecutive errors, after which he refused to respond even after long adaptation. I then turned the slit back to $\lambda = 5480$ (green of original training series) and gave him 5 trials; all were correct and the average time of reaction was 2 seconds.

Chick R chose twice correctly and then made three consecutive errors. When tested on green of training series, 5 correct choices were made in an average time of 2 seconds.

Chick G would give only 8 responses; during these 8 trials two errors were made. The animal chose with great difficulty. The average time of this chick

*When it appeared it came in a burst of red light. There was no photochromatic interval whatever.

was 12 seconds. Since it had made but one error during the whole course of the experiment, and that on the third day of its preliminary trials, I think it fair to call this place $\lambda = 7150$, the red limit under the conditions of the present test. The limit of D is evidently $\lambda = 7000$, while that of R is between $\lambda = 7000$ and $\lambda = 7150$. When tested with light-waves of greater length, no response was obtained.

LIMITS AT THE VIOLET END.

After a preliminary test upon green $\lambda = 5480$, in which all choices were correct, the chicks were tested with violet $\lambda = 4500$, with the Nernst as a source. The stimulus was plainly visible to me. Six tests were given each animal, as shown in table 27:

TABLE 27.

	No. of trials.	Animal.	Per cent correct.	Remarks.
$\lambda = 4500$	6	D	100	Clearly above my threshold the moment the light was turned out.
	6	R	100	
	6	G	83	
$\lambda = 4200$	8	D	82	Animal seemed to experience great difficulty. Light above my threshold at end of the 5 seconds.
	8	R	100	
	8	G	100	

Again I became suspicous as to the adequate stimulus, especially in view of Hess's work. In order to eliminate all diffuse and scattered white light I put in a filter of $CuSO_4$ to which an excess of ammonia has been added immediately behind the selecting slit. The thickness of the layer was 1 cm. I first tested with a pocket spectrometer, to see that only violet and bluish-violet rays were admitted. It was necessary to open up the slits, S_1 to 0.75 mm. and S_2 to 1 mm., in order to make the light visible to me. The results are given in table 28.

In order to obtain fairly high intensities in the shorter wave-lengths I found it necessary to substitute the automatic Bausch and Lomb arc lamp for the Nernst.* The filter was arranged as before. The results appear in table 29.

TABLE 28.

$\lambda = 4500$.			
No. of trials.	Animal.	Per cent correct.	Remarks.
8	D	88	Clearly over my threshold at end of 2 seconds.
8	R	100	
8	G	100	

TABLE 29.

$\lambda = 4010$.		
No. of trials.	Animal.	Per cent correct.
8	D	100
8	R	100
8	G	100

On the following day the arc was again used, but as an extra precaution against white light I placed a filter of the same constitution as the above (cupra-ammonium) in front of the slit in the collimator S_1. The pocket spectrometer showed that no light was admitted except the violet rays.

The chicks were first tested with $\lambda = 3950$. It was visible to me at the end of a long adaptation time, but not visible under the conditions offered the chicks.

*Yerkes and Watson, *op. cit.*, p. 89.

Both D and R broke down completely and took up right-hand position habits. I then immediately gave them $\lambda = 4050$, with conditions as in previous tests, with the results shown in table 30.

Evidently under conditions such as I have been describing the violet limit lies between $\lambda = 4050$ and $\lambda = 3950$ for these two animals. The stimulation was in every case over my threshold. Furthermore, the animals were admitted 10 seconds instead of 5 after turning out the light.

Much to my surprise chick G gave different results. He was tested under the same conditions as D and R with $\lambda = 3950$. The light to which he responded was never above my threshold. I give his record in full in table 31. In every trial in which 10 seconds were given, the animal responded correctly. It is evident that the threshold is a flexible kind of thing and dependent naturally upon adaptation. The limit for G for long adaptation is certainly lower than $\lambda = 3950$, but for 5 seconds' adaptation at the present intensity $\lambda = 3950$ is about the limit.

Table 30.

$\lambda = 4050$.		
No. of trials.	Animal.	Per cent correct.
6	D	100
6	R	100

On the whole, I think it perfectly safe to conclude that chicks do not have a spectrum shortened at the violet end, nor yet do they show any special sensitivity in the violet. The experiments of Hess* and of Katz and Révész† lead them to believe that the chick is practically blind to blue and violet rays.

Table 31.

Chick G. $\lambda = 3950$.			
No. of trials.	Position of light.	Animal's response.	Remarks.
1	Left	Left	10 seconds after turning out light.
2	Left	Left	
3	Right	Right	
4	Right	Right	
5	Right	Right	
6	Left	Left	
7	Left	Right	5 seconds after turning out light.
8	Right	Right	10 seconds after turning out light.
9	Left	Left	
10	Left	Left	
11	Right	Left	5 seconds after turning out light.

As will be seen by the later experiments reported in this paper, the chicks are probably as much stimulated by these rays as the human being. It is barely possible that the difference in the strain of chick used is responsible for the differences in our results. I think, however, that their method of allowing the chick to pick up grains of food illuminated by the different rays is a faulty

*Hess, C. Untersuchungen über Lichtsinn und Farbensinn der Tagvogel. Archiv f. Augenkeilkunder, Bd. 57, S. 317.

†Katz und Révész. Experimentell-Psychologische Untersuchungen mit Huhner. Zeit. f. Psy., 1909, S. 93.

one. In my experiments, where a large stimulating surface was used, the question of visual acuity does not enter in. It does unquestionably in their work. Furthermore, their work requires a reaction which runs counter to the chicks' ordinary habits. The birds in daily life constantly have occasion to pick up greenish-yellow grains, yellow grains, and reddish-yellow grains, but rarely or never blue and violet grains.

RELATIVE STIMULATING EFFECT OF MONOCHROMATIC LIGHT.

While at work upon the limits of the spectrum, it occurred to me that, since the chicks were already trained by this work on the extremes of the spectrum to respond to faint stimulation by light, it would be advisable to extend the scope of the experiment so as to obtain the curve of the relative stimulating effect of light of different wave-lengths. Such a curve has never been obtained hitherto on animals. It has been worked out for man under several sets of conditions—for the light-adapted eye, for the dark-adapted eye, and for eyes possessing various abnormalities in sensitivity to monochromatic light. This work, even on man, has been done in general with very poor physical controls. There is general agreement that the maximal sensitivity for the light-adapted eye lies in the region of the yellow-green, and for the dark-adapted eye in the bluish-green.

Furthermore, it is desirable to test the question whether the course of such a curve, when obtained in the chick, will be like that found in man. Comparison can be made only when the data for the two curves have been obtained under similar conditions.

Our procedure in obtaining the curves shown below was as follows: We first obtained refinement of the monochromatic light by the use of the auxiliary spectrometer (p. 88). Each monochromatic bundle before being admitted to the dark room was made to bear a standard amount of energy. The thresholds for the animals were then obtained by cutting down the energy of the light by means of the Brodhun sector. Since the different bundles worked with all have the same initial energy when admitted, the sensitivity curve can be plotted throughout the spectrum directly from the angular opening of the sector just sufficient to produce a response at the given wave-length, or as is more usual from the reciprocals of these values. In the curve these sector values are averaged for the three chicks.

The method of obtaining the chick's response was similar to that used in the work on limits. The apparatus was set for work at a given wave-length. The motor driving the Brodhun sector was started and left running during the whole course of the experiment. The intensity of the light falling on the stimulus patch was at first high (full 140 mm. of the Brodhun sector, but with the smoked glass in place). The chicks were given 10 minutes' complete darkness adaptation, then the tantalum light was turned up for 5 seconds, then came a wait of 5 seconds in complete darkness. The door to the reaction chamber was next raised and the chicks were allowed to respond. The stimulus on the first trial was always well above the threshold. On the second trial the angular opening was made considerably smaller; and on the succeeding trials, still further decreased, until at last a break-down occurred, *i. e.*, the chick refused to respond, or else waited long past the usual time for response, for adaptation to increase the stimulating effect of the light. I tried to regulate

the steps so that a satisfactory value could be obtained and verified in the course of 12 to 16 trials. In several cases where doubt of the correct setting of the sector was possible, I delayed advancing on the following day to a new wave-length, in order to be able to repeat the experiments. It was not deemed advisable to use an ascending series of steps in this work, *i. e.*, to start below the threshold and gradually increase the intensity of the light until positive response was obtained (for fear of onset of position habits). If more time could have been given over to the experiment such a technique would unquestionably have been possible.

After the day's experiments upon the three chicks were over (the experiment lasted roughly an hour) my own threshold was taken. My wife adjusted the sector. The procedure was as follows: My eye was kept 1 meter from the plaster surface; the tantalum light was turned up for 5 seconds. While it was on I glanced at a neutral gray paper during the first 4 seconds (in order to avoid strong after-image). On the fifth count (metronome) I again fixated the plaster surface and shut my eyes while the light was turned out for 5 seconds. On the sixth second I opened my eyes and observed the plaster surface. If it were visible the experiment was repeated until the plaster surface was not visible immediately upon opening my eyes. This was repeated five times with ascending series and five times with descending series and the results were averaged.

Table 32 shows the actual sector openings for chick and man and the reciprocals of those values.

TABLE 32.

Wave-length.	In chick.		In man.	
	Sector opening.	Reciprocal.	Sector opening.	Reciprocal.
*660$\mu\mu$	870.00 mm.	.0011	900.00 mm.	.00111
600	10.72	.0932	7.36	.1358
580	9.90	.1010	2.82	.3546
550	1.52	.6578	.54	1.8518
520	1.40	.7142	.42	2.3809
480	1.98	.5050	.95	1.0526
452	2.66	.3759	1.82	.5494

The curve (fig. 10) is plotted from the reciprocals. It is clear that the higher the energy required for any given monochromatic light to stimulate the retina, the less the sensitivity of the retina to that light; that is, the reciprocals of the energy value ($1/e$, where e equals the energy necessary for stimulation) will, when plotted against the wave-length, give the usual form of a sensitivity curve.

In examining the curve it is well to remember that the higher the point on the curve is above the base line, the less is the energy required to produce stimulation. In this work we do not know in absolute terms (ergs) the standard energy carried by each beam with which we worked, but we do know that all beams had the same initial energy and consequently that the final sector

*The opening at 660 $\mu\mu$ is of course merely proportional. At all other wave-lengths a smoked wedge was used after the energy was calibrated, the transmission curve of which, from 396 $\mu\mu$ to 760 $\mu\mu$, was known (calibrated by Bureau of Standards). At 660 $\mu\mu$ the efficiency was so low that the wedge had to be removed.

opening at which the various thresholds were obtained are all directly comparable and proportional to the absolute energy required for stimulation.

The relative stimulating effect of the different regions of the spectrum for the (partially) dark-adapted eye appears clearly in the curve. The maximum efficiency for both chick and man is found at $\lambda = 5300$, which agrees fairly closely with what previous work we have upon man.* From the maximal point one finds a decrease in efficiency in passing toward either end. It is unfortunate that the calibrating of the selenium cell did not enable us to make tests with wave-lengths shorter than $\lambda = 4500$. The decrease in efficiency is enormous as one reaches the red. While in every region except red the curve of the human lies higher above the base-line than that of the birds, it is questionable whether the human has a lower absolute threshold than the chick. The method of obtaining the threshold of response for the chick here used was a rapid one. If more care had been taken and the absolute threshold had been desired, there is little room for doubt that the curve for the chick would more nearly coincide with that of man. I draw this conclusion from numerous chance experiments, some of which are cited in the text. A separate investigation for each wave-length would be required to thoroughly work out the absolute threshold values for chick and man. At $\lambda = 6600$ the present curve shows that the threshold of stimulation is lower in the chick than in man.

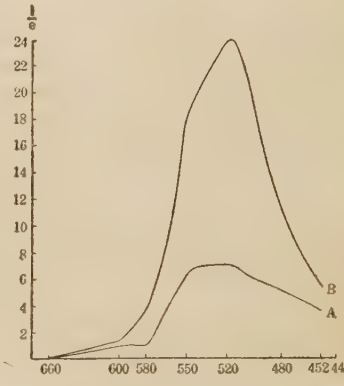


FIG. 10.—Sensitivity Curve (dark-adapted eye). A, chick; B, man. The ordinate shows the reciprocal (1 mm. = .01) of energy value necessary to produce reaction at the wave-length given on the abscissa.

RÉSUMÉ OF RESULTS OF STUDY ON CHICK.

(1) The limit of the chick's spectrum at the red end lies probably between $\lambda = 7000$ and $\lambda = 7150$; at the violet end between $\lambda = 3950$ and $\lambda = 4050$. The limits for the human eye have been found to lie between $\lambda = 4000$ and $\lambda = 7600$. These limits are flexible and depend somewhat on the absolute intensity of the light and necessarily upon the state of adaptation. Beyond these limits, visual responses are completely lacking. Unless the retinas of other day birds show sensitivity in the red far beyond the point at which response ceases in the chick, any theory to account for homing such as that of Duchâtel remains without scientific support.

(2) The curves for man and chick showing the relative stimulating effect of different regions are similar throughout their course, except in the extreme red.

*See the work and historical summary of A. Pflüger, Annalen der Physik, 9 (1902), pp. 185–208. As will be seen, the curve obtained from the human subject here used shows only one maximum. In most of Pflüger's work there is evidence of a secondary maximum. While his work was apparently much more carefully controlled than my own, I am convinced that the secondary maximum obtained by him is due to some failure properly to control the energy of the stimulus

LIMITS OF SPECTRAL SENSITIVITY IN THE HOMING PIGEON.

At the time of completing the foregoing experiments upon the chick no homing pigeons of suitable age and stock could be found. In the fall of 1914 work was begun upon 4 young homing pigeons whose parents had "flight diplomas," but only one of them proved suitable for work. All were purchased when 3 weeks of age and were immediately given the preliminary exercises in the box shown on page 89. We experienced great difficulty in getting any of the pigeons to form the habit of passing completely around to the food. One pigeon, after several months of preliminary practice, during which tests were given daily, began to work steadily. By January 10, 1915, controlled experiments were begun. This bird was trained on yellow and green. It was found that while the pigeon was positive to light, it was not so automatic in its responses as the chick. After sufficient training, however, this pigeon became as invariable in its reactions to the light as the chicks. In the following account the tests on the intermediate parts of the spectrum are only summarized. As we approach the limits at the two ends the tests are given in detail.

LIMITS AT THE VIOLET END.

January 10. Eight tests on $\lambda = 5500$ were made. Only 1 error appeared.*

January 12. Eight tests on $\lambda = 5060$ were given, with no errors.

January 15. At wave-length $\lambda = 4780$: with slit in collimator open to 1 mm. and slit in objective open to 1.5 mm., the following tests were given, *without previously dark-adapting the pigeon* or allowing a period of adaptation after light was turned out (for method see p. 90). This test was made to get rapid orientation as to sensitivity of pigeon to light rays of short wave-length. The results were:

(1) Light on left side, wrong response.
(2) Light on left side, wrong response.
(3) Light on right side, right response.
(4) Light on left side, wrong response.
(5) Light on left side, wrong response.

Since a right position habit developed, I broke off the tests. It is evident that under the given conditions the light no longer controls the response. From this it would seem clear that there is no extreme sensitivity to violet rays.

January 16. Wave-length decreased to $\lambda = 4680$. *Animal was dark-adapted for 15 minutes,* and *30 seconds adaptation allowed before light was exposed.* Complete record as follows, with a result of 77+ per cent correct:

(1) Light on left, wrong response.
(2) Light on left, right response.
(3) Light on right, right response.
(4) Light on right, right response.
(5) Light on left, right response.
(6) Light on left, right response.
(7) Light on right, right response.
(8) Light on right, right response.
(9) Light on left, wrong response.

Under conditions of the above test it is evident that although the limit is not yet reached, the light has very low stimulating value.

January 17. Wave-length changed to $\lambda = 4580$; 15 minutes adaptation and 30 seconds adaptation before admitting light. 88+ per cent correct.

(1) Light on right, response right.
(2) Light on left, response right.
(3) Light on left, response right.
(4) Light on left, response right.
(5) Light on left, response right.
(6) Light on right, response wrong.
(7) Light on right, response right.
(8) Light on left, response right.
(9) Light on right, response right.

*In all of the tests recorded here the purified spectrum of Helmholtz was used (p. 88). Unless otherwise noted, a Nernst glower burning at a constant amperage was used as a source. For general illumination (during feeding, etc.) a 40-watt tungsten was substituted for the tantalum light of the previous paper.

The responses were exceedingly rapid. The one error was probably caused by giving 4 tests on left side, and then changing.

January 18. By referring to the work on the chick it will be seen that the standard condition was 10 minutes dark adaptation and 5 seconds adaptation before exposing light. In the test below the slit in the collimator (S_1) was narrowed to 0.5 mm. and the slit in the objective (S_2) to 1 mm. Wave-length was changed to $\lambda = 4350$. Under these more difficult conditions the results were 28+ per cent correct.

(1) Light on left, response wrong.
(2) Light on left, response right.
(3) Light on right, response wrong.
(4) Light on right, response wrong.
(5) Light on left, response right.
(6) Light on right, response wrong.
(7) Light on right, response wrong.

Under such conditions the stimulating effect is not sufficient to control reactions. In this case the light was not visible to me, but it became visible when 17 seconds adaptation was allowed between turning out of light and exposure of stimulus.

January 19. Animal was again tested under same conditions, except that I waited before admitting animal until light became *clearly* visible to me. The seconds noted in first column give periods of adaptation before admitting animal. Results:

50 sec. Light on right, response right.
35 sec. Light on left, response right.
45 sec. Light on right, response right.
40 sec. Light on right, response wrong.
30 sec. Light on right, response right.
35 sec. Light on left, response right.
30 sec. Light on right, response wrong.
40 sec. Light on right, response wrong.
40 sec. Light on right, response wrong.

Since left position habit was setting in, I next widened slit in collimator to 1 mm. and allowed 60 seconds for adaptation. Light was visible to me at end of 10 seconds.

Light on right, response wrong.

I next opened slit in objective to 2 mm. (light visible to me at end of 4 seconds) and allowed 60 seconds adaptation.

Light on right, response right.
Light on right, response wrong.

It is evident that we are working here on threshold at the present intensity. Again the tests show clearly that there is no very great sensitivity to the violet—that the pigeon's retina is even less sensitive than the human retina to those rays.

January 20. I next tried the effect of greatly increasing the intensity of the light. The automatic B. & L. arc was used as a source instead of the Nernst: slit in collimator changed to 0.5 mm., slit in objective to 1 mm. The light so obtained was clearly over my threshold the moment it was exposed. Results:

(1) Light on right, response right.
(2) Light on right, response right.
(3) Light on left, response right.
(4) Light on left, response right.
(5) Light on right, response wrong.
(6) Light on right, response wrong.

Up to this point a 30 seconds adaptation period was given. Since the last two tests gave wrong responses, I increased adaptation to 60 seconds.

(7) Light on right, response right.
(8) Light on left, response right.
(9) Light on right, response right.
(10) Light on left, response right.

In this series it is clear that the limit of spectral sensibility has not yet been reached.

January 21. In order that we may readily reach the limits with the arc, I changed wavelength to $\lambda = 4050$, with all other conditions as in the test on January 19, except that the slit in the collimator was opened to 0.75 mm.

25 sec. Light on left, response right.
30 sec. Light on right, response right. Hesitated 20 sec. before starting.
27 sec. Light right, response wrong. Hesitated 15 sec.
40 sec. Light right, response wrong. Hesitated 15 sec.
40 sec. Light on left, response right. Evident that left position habit is forming.
30 sec. Light on right, response wrong.
55 sec. Light on right, response right.
35 sec. Light on left, response right. Hesitated 22 sec., then first went wrong and then turned to left.
30 sec. Light right, response wrong. Hesitated 25 sec.
33 sec. Light right, response wrong.
30 sec. Light left, response right.

On analysis it will be found that on all of the trials except two the bird went to the left side. As before, the first column shows the number of seconds required to bring the light *clearly* over my threshold. Since this was the greatest intensity of light which I could obtain with any purity, and since, further, the results seem reasonably clear, I decided not to make further tests on the violet region for fear of setting up a troublesome position habit (which would interfere with the work in the red region). The above tests show without further discussion that $\lambda = 4050$ under the given conditions is beyond the pigeon's limit of spectral sensitivity. Since the tests on January 19 show that at highest intensity $\lambda = 4350$ is not beyond the limits, it follows that the limit lies somewhere between $\lambda = 4350$ and $\lambda = 4050$. The pigeon is somewhat less sensitive to violet rays than is either man or chick. To completely work out this decrease in sensitivity in quantitative terms, while a fruitful problem, is beyond the scope of the present work.

LIMITS AT THE RED END.

January 22. In the tests on the sensitivity in the red region the Nernst was again used as the source. The slit in the collimator was set at 0.5 mm. and in the objective at 1 mm.; 10 minutes adaptation before beginning the tests was allowed and 10 seconds adaptation was allowed on each trial after the light was turned out before exposing stimulus light. Reading: $\lambda = 6350$.

(1) Color right, response right.
(2) Color right, response wrong.
(3) Color left, response right.
(4) Color right, response right.
(5) Color right, response right.
(6) Color left, response right.
(7) Color right, response right.
(8) Color right, response right.
(9) Color left, response right.
(10) Color left, response right.
(11) Color right, response right.

Has broken position habit carried over from violet. Animal hesitated at first and did not seem eager to work.

We thus see that changing directly from violet end, where left position habit was formed, to red end, resulted in the breaking of position habit. This confirms our results of the previous day in a striking way. The light now lying within the bird's range again controls the response.

January 23. $\lambda = 6840$; otherwise conditions same as above, except that slit in the collimator was opened to 1 mm. 63 per cent correct.

28* sec. Light right, response right.
18 sec. Light left, response wrong.
18 sec. Light left, response right.
20 sec. Light right, response wrong.
22 sec. Light right, response right.
18 sec. Light left, response right.
16 sec. Light left, response right.
20 sec. Light right, response wrong.
22 sec. Light right, response right.
20 sec. Light left, response right.
25 sec. Light left, response wrong.

*First column shows time required for the observer to see light. Stimulus light was then exposed to animal.

Under these conditions we find evidence that the light is exerting only a slight stimulating effect. While the responses are only 63 per cent correct, the fact that no pronounced position habit developed is the most significant evidence that the light lies within the animal's range.

January 24. I tried the pigeon on the same wave-length, but increased the intensity of the spectrum by using the arc light as a source. The slit in the collimator was closed to 0.5 mm. The light was over my threshold (with light-adapted eye) the moment it was exposed. Results:

Light left, response right.
Light right, response wrong.
Light right, response wrong.

In the above tests, since the light was so intense, I allowed only 10 seconds adaptation. Since a left position habit was forming, I increased the adaptation period to 30 seconds. 70 per cent correct.

Light right, response right.
Light left, response right.
Light right, response wrong.
Light right, response right.
Light right, response right.
Light right, response wrong.

Light left, response right.
Light right, response right.
Light left, response right.
Light right, response wrong.
Light right, response wrong.

Since there was a left-position tendency growing I decided to *retrain* on $\lambda = 5800$ (which was employed in the early training work).

January 25. Nernst lamp is the source. Slit in collimator 0.75 mm. Object of test, to see if position habit carried over to a situation where the stimulus was known to be effective. $\lambda = 5800$. Responses, 70 per cent correct.

Light right, response right.
Light left, response right.
Light right, response wrong.
Light right, response right.
Light right, response wrong.
Light right, response right.
Light right, response wrong.
Light right, response right.
Light right, response wrong.

Light left, response right.
Light right, response right.
Light right, response right.
Light left, response right.
Light right, response wrong.
Light right, response right.
Light right, response right.
Light right, response right.
Light right, response wrong.

This was an extremely difficult series in view of the fact that all but three trials were given on the right side. Even granting this, it is obvious that animal needs further training to remove position habit.

January 26. Conditions as above.

Light right, response right.
Light left, response right.
Light right, response wrong.

Light right, response right.
Light left, response wrong.

The last trial shows that left position habit has given way and that the large number of trials given on the right during the preceding day is tending to give a *right* position habit. From now on light alone should control response. That such is the case appears from the results of the remainder of the test, which gave 90 per cent correct.

Light right, response right.
Light right, response right.
Light left, response right.
Light right, response wrong.
Light right, response right.

Light left, response right.
Light right, response right.
Light right, response right.
Light left, response right.
Light left, response right.

The position habit has been corrected and the animal is once more automatic in its responses.

January 27. $\lambda = 7120$. Arc lamp; other conditions as on January 24. In to-day's series instead of again trying animal on $\lambda = 6840$, at which point a position habit began to appear, I gave light somewhat further along, $\lambda = 7120$.

After adjusting the intensity of the light so that I could see it (10 minutes adaptation) 20 seconds after tungsten light was turned off, the following results were obtained; 100 per cent correct:

Light left, response right.
Light right, response right.
Light left, response right.
Light right, response right.
Light left, response right.

In the above I had the slit in the collimator at 0.75 mm., which is too wide of course to give a very "pure" spectrum. I next narrowed this slit to 0.5 mm.

Light right, response right.
Light left, response wrong.
Light left, response right.

In the three trials last considered the animal began to wait from 1 to 3 minutes to respond. Fearing another breakdown, I again opened the slit to 0.75 mm.

Light right, response right.
Light left, response right.
Light right, response right.
Light right, response wrong.
Light right, response right.
Light left, response right.
Light left, response right.

There can be but little question of sensitivity here when the intensity is great. The breakdown at $\lambda = 6848$ was unquestionably due to the position habit. In the present series, the slit is so wide that it can not be determined whether the animal is reacting to the wave-length 7120 or to those slightly shorter which are undoubtedly present in the beam. It is hardly worth while to try to determine the limits any more accurately. The extreme red rays have little stimulating effect and the slits have to be opened wide in order to obtain a ray of sufficient intensity to call out response. Under such conditions we can no longer guarantee the purity of the light. One crucial test remains, however, viz, to give the animal the opportunity to react to rays beyond the (human) visible spectrum.

January 28. $\lambda = 8000$. Arc lamp as source. Collimator slit opened to 1 mm. and slit in objective to 1.25. The wave admitted ranged probably from 7700 to 8100. That the infra-red rays were falling upon the plaster surface was determined by the use of the selenium cell. No light was visible on plaster surface to me (or to Dr. Lashley) even after long darkness adaptation. Large deflections in the galvanometer were noticed whenever the selenium cell was brought in immediately above the plaster surface. One series of 10 trials was given. The order of the series was determined in advance by the throwing of a die. The results were as follows:

Light right, response wrong.
Light left, response right.
Light right, response wrong.
Light right, response wrong.
Light left, response right.
Light right, response wrong.
Light right, response wrong.
Light right, response wrong.
Light left, response right.
Light left, response right.

Again we see the onset of a pronounced position habit. The animal *went to left side on every trial, regardless of position of light.*

I then gave the animal immediately $\lambda = 5700$.

Light right, response wrong.

The position habit was carried over for this one trial, then gave way as follows, 100 per cent correct:

Light right, response right.	Light right, response right.
Light right, response right.	Light left, response right.
Light right, response right.	Light right, response right.
Light left, response right.	Light left, response right.
Light right, response right.	Light right, response right.

The results from all of the tests seem conclusive to the author. Somewhere in the neighborhood of $\lambda = 7120$, under the conditions of the present test, we find the limits of sensitivity. This is practically the limit we obtained for the chick (see p. 94).

RÉSUMÉ OF TESTS ON PIGEON.

(1) The range of spectral sensitivity in the homing pigeon lies approximately between $\lambda = 4200$ and $\lambda = 7120$. It was not our purpose to determine the limits with any great degree of exactness, since they are dependent to some extent upon adaptation and upon the absolute intensity of the ray of light exposed.

(2) The homing pigeon is not sensitive to infra-red rays, at least to those which have approximately a length of $\lambda = 8000$. Since neither a quartz nor a rock-salt prism was at our disposal we could not give an ultra-violet light of any intensity. The evidence on the violet end is clear, however, in view of the fact that the efficiency of the violet rays falls off apparently somewhat more rapidly for the pigeon than even for man.

(3) Hess is unquestionably wrong when he states that the pigeon is "blind to blue and violet." We have shown the contrary quite clearly, even in the notes which we have presented in this paper. Our unpublished data show that this animal is sensitive to all wave-lengths between the limits expressed in (1) above.

(4) The young pigeon is positive to light, but is not so automatic in its responses as the young chick.

THE ACQUISITION OF SKILL IN ARCHERY.

By K. S. LASHLEY.

EQUIPMENT FOR ARCHERY.

Fig. 1. Homewood target as it appeared to the archers. Fig. 2. Practice shed with equipment.
Fig. 3. Target arranged for rifle practice.

THE ACQUISITION OF SKILL IN ARCHERY.

The great majority of experiments upon habit formation in man have dealt with functions involved in implicit behavior; functions connected chiefly with the speech mechanisms have received most attention. Improvement in such activities as addition, translation into code or from code, solving mechanical puzzles, checking letters on a printed page, or memorizing word material is due almost wholly to increase in complexity of language habits. Practice in typewriting and musical technique is, in the beginning, followed by the formation of implicit* habits which later are dropped out to leave only the direct sensory-motor reactions from eye to fingers. Improvement in writing and in telegraphy probably involve similar changes. In the study of all such functions in human adults the subjects have already, at the beginning of the experiments, a vast number of constellations of habits in the implicit systems which have more or less in common with the problem offered by the experimental situation and may influence the course of learning in unknown ways. This is illustrated by the relation between the fullness of "meaning" of word material (*i. e.*, the number of habits in which the material is already involved) and the ease with which it is learned.

The existence of such complex systems of implicit behavior with their equally complex relations to overt activity makes it difficult to distinguish the different functions improved in learning, or to say whether a given amount of improvement is the result of a gradual coordination of many unrelated habits or of the simpler union of a few constellations of habits; whether the practice is distributed over a large number of nervous changes or is concentrated upon the fixation of a very few new neural pathways. For an insight into the mechanism of habit formation some simple, more direct sensory-motor associations must be studied, particularly such as permit of the control of related functions, metabolism, etc. This is best accomplished with animals, as they may be subjected to more vigorous training methods than is possible with man, but some types of learning in man, free from the complications of language habits, must be studied before the extension of the results obtained with animals to man will be completely justified. The activities studied by Bair (tossing shot), Whitley (tracing the smooth maze), Swift (tossing balls), Wells (tapping), and Partridge (inhibition of the winking reflex) call for the formation of relatively simple motor habits, but have the disadvantage of giving little to interest the subjects and probably offer a weaker stimulus to learning than is provided by more complex activities.

In the spring of 1913 Dr. J. B. Watson suggested archery as a means of studying habit formation in man which would in part avoid the complexity of language habits. Material for practice in archery was obtained and a preliminary experiment was begun by him at the Marine Biological Laboratory of the Carnegie Institution of Washington on Loggerhead Key, Tortugas. The

*The term "implicit" is used in the sense defined by Watson (1914) as a common name for the movements, too slight for detection, which seem to bridge the gap between external stimulus and overt reactions when more than a single reflex is involved.

chief object of this experiment was to test the value of the method and to work out the technique of experimentation, but the range of the experiment was extended to include a comparison of the rate of improvement in two groups of men differing in certain respects, to be considered later. The writer acted as subject and assisted in conducting this experiment. During the spring of 1914 the range described below was constructed on the grounds of The Johns Hopkins University and the experiments were continued in a study of the relation of the rate of improvement to the distribution of practice.

I wish to express my very great indebtedness to Professor Watson for the unrestricted use of the data obtained at the Tortugas, for the main features in the design of the apparatus employed in the later work, and particularly for his generous advice and assistance throughout the experiments.

My thanks are due also to Dr. Alfred G. Mayer for his interest in the work and for permitting his laboratory staff to devote their time to it.

Finally, I owe a great deal to the people who have taken part in the experiments, particularly to those in the "5-shot group," whose regular practice for many weeks was carried out at no small inconvenience to themselves.

EQUIPMENT.

The equipment used in the Tortugas experiments consisted of bows, arrows, target, screen to mark the results of wild shots, and a shield for the experimenter near the target. The arrows were of good quality, 28 inches in length. The bows were of lemon wood, 6 feet in length, and requiring a pull of 44 pounds to draw the arrows to the head. An arrow properly discharged from such a bow will pierce a half-inch pine board at a distance of 40 yards and has an almost flat trajectory for that distance. The target was the official 48-inch target of rye straw faced with white canvas, on which was painted a 10-inch black bulls-eye. It was mounted on a metal tripod so that the center of the bulls-eye was 4 feet from the ground.

The length of the range used in all the experiments was 40 yards, at which distance the bulls-eye subtends a visual angle of 33′. The target was set up at the foot of a sand-bank, which served to stop the arrows passing the target. As beginners rarely hit the target at this distance, some method of recording the distance of wild shots from the bulls-eye was necessary. For this purpose a muslin screen, 14 feet square, was erected around the target. The arrows made holes in the cloth and the distance from these to the center of the bulls-eye was taken as the record of the shots. This made it necessary to measure each shot as soon as it was fired, a method which proved neither convenient nor safe.

The method employed in the first experiment was the following: The subject was given instruction by word and example of the method of nocking, drawing, and loosing the arrow, but was told nothing of the method of aiming, the choice of a point of aim, or the general bodily position in aiming and loosing. He had to find out for himself how to aim, to prevent the bowstring from catching on his clothing or arm, to allow for the curve of the arrow's trajectory, to counteract the kick of the bow by increased tonus of the flexors of the left arm, together with the large number of finer details which make for accuracy. After the first instruction he was given a bow and 12 arrows and sent to a point 40 yards in front of the target, where he stood alone and loosed the arrows

at intervals of 2 minutes, or in each case as soon as the preceding shot had been measured and recorded. Twelve shots were fired thus each day for 30 days. The subjects had no opportunity to observe each others' methods of shooting from a distance of less than 40 yards. Discussion of methods of shooting was prohibited, but not with perfect success. The result of each man's practice was posted daily and for group B (page 111) a prize of $5 was offered to the subject making the best score on the greatest number of days.

For the continuation of the work in Baltimore a safer and more convenient range was necessary. A plot of open ground, 40 yards in length by 8 in width, was laid out and the buildings shown in plate 8, figures 1, 2, and 3, were put up at opposite ends of this. The small shed (figure 2), which serves to protect and isolate the subject, is 12 feet high and 12 feet wide by 8 feet deep. It is equipped with racks for arrows, bows, etc., and a small telescope for use in rifle practice. The target shed is somewhat larger, 14 feet high by 12 by 12 feet (plate 8, figures 1 and 3). Three feet back from the open front of this shed is a pile of baled straw, packed tightly from the floor to the roof. The space behind this straw partition is used for the storage of extra equipment. The front of the shed is provided with large swinging doors, padded on the inside with heavy quilted canvas mats. When these are opened, as in the photographs, a surface 14 by 24 feet is exposed, which will catch and hold the arrows wherever they strike. The 4-foot straw target is mounted in the center of the baled straw and a 10-inch paper bulls-eye marks the center of this. Plate 8, figure 1, shows the appearance of the target-shed from the opposite end of the range.

In the main experiment rifle practice was used as an index of the relative ability of the subjects under the same conditions of practice. An efficient single-shot air-rifle was used at a range of 40 yards. The 4-foot steel-faced target used with this is shown in plate 8, figure 3. A constant supply of paper targets was provided, a separate one being used for each subject's daily practice. The entire equipment could be swung out of the way readily in preparation for arrow practice. As the bullet holes in the paper are not clearly visible at 40 yards it was necessary for the experimenter to watch the target through a telescope and report the position of each shot to the subjects.

COMPUTATION OF AVERAGES AND COMPARISON OF INDIVIDUAL AND GROUP RECORDS.

A series of records of shots with the bow or rifle invariably shows a great range in the accuracy of the shots, even in the case of subjects who have acquired a considerable degree of skill. With increasing skill the range of variation decreases, but the relative amount of variation as measured by the coefficient of variation remains practically constant. This is illustrated by the record of one subject given in table 33.

Some of this variation may be the result of the learning process itself (the successive trial of different methods), but the greater part of it is certainly due to chance external agents, such as variations in the strength of the wind, weight of the arrows, tightness of the nock on the string, tension of the bow, temperature, and extent of fatigue. As a result the average of any small number of shots does not give a fair measure of the skill of the individual, and the use of such averages as a measure of the initial and final accuracy and amount of improvement does not lead to trustworthy results.

All group records, each based upon 20 successive shots, are included in table 39, but for the computation of the amount of improvement it has seemed best to use averages based upon as large a number of shots as possible. The simplest method, and the one which probably gives the most dependable results where comparisons are made of the total amount of improvement in a given amount of practice, is that of using the average of all shots during the first half of practice as an index of the initial accuracy, and the average of all during the second half as an index of final accuracy.

TABLE 33.—*Relative amount of variation in records of shots during successive practice periods for one subject of the 12-shot group.*

Av. distance from bulls-eye.	Av. variation.	Av. coefficient of variation.	Av. distance from bulls-eye.	Av. variation.	Av. coefficient of variation.	Av. distance from bulls-eye.	Av. variation.	Av. coefficient of variation.
inches.	*inches.*	*per cent.*	*inches.*	*inches.*	*per cent.*	*inches.*	*inches.*	*per cent.*
109.0	31.8	0.28	43.8	24.2	0.55	38.5	8.0	0.21
79.9	15.4	.19	43.9	13.1	.30	40.1	10.7	.27
48.3	22.4	.42	49.6	16.6	.33	39.9	9.4	.24
46.6	20.8	.45	47.3	29.9	.63	29.5	7.3	.25
49.7	19.3	.39	40.2	15.4	.38	28.1	9.0	.32
72.3	35.6	.49	41.8	15.3	.37	39.0	13.2	.34
53.5	24.2	.45	38.4	10.8	.28	39.3	14.8	.38
52.0	19.3	.37	29.5	10.0	.34	29.6	15.2	.51
46.6	21.0	.45	39.2	11.3	.29	41.0	11.4	.28
54.2	25.5	.47	34.5	10.8	.31	33.7	15.5	.46

The chief objection to this method is that it assumes a like form for the learning curves of the groups compared and does not give sufficient weight to rapid initial improvement, final spurts, or similar variations in the rate of learning. To meet this objection the improvement from the first to the last 5 and from the first to the last 40 shots has also been computed and compared with that obtained by the first method.

In comparative studies of learning, the further question of the relative value of the absolute amount of improvement and the percentage improvement, in terms of initial skill, arises. It seems to the writer that the use of percentages is justified only when it is clear that each step in learning presents the same difficulty to all individuals compared. If the first stages of learning present, for the individual with a low initial skill, the same difficulty as do the second stages for the individual with greater initial skill, then the absolute improvement would seem a more accurate measure for the effects of such factors as distribution of practice. Figure 11 shows the relations of initial and final skill in archery for the individuals studied. On the average, the absolute improvement of those of low initial accuracy was little greater than that of the others, which indicates that the problem really presented greater difficulties for them throughout.

In the case of rifle practice this does not seem to be true. Figure 12 shows that individuals of low initial accuracy made by far the greater absolute improvement, and in this case it seems that the percentage rather than the absolute improvement should be used in comparison of the groups.

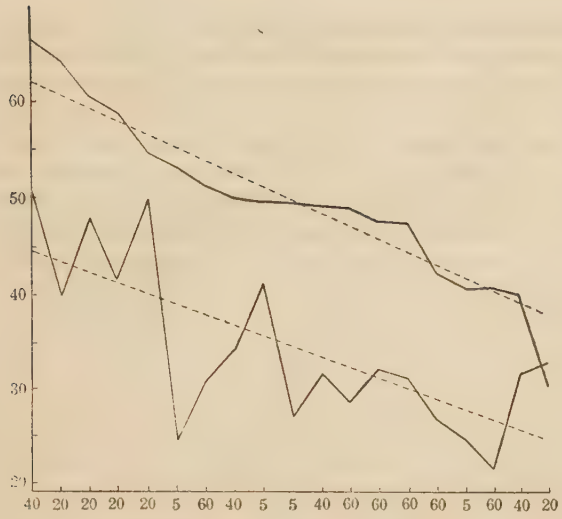


FIG. 11.—Initial and final accuracy with the bow in 19 subjects computed from first and last 40 shots. The ordinates are *inches from the bulls-eye;* the abscissæ, the different subjects arranged in the order of their initial skill. The groups of which they are members are given below. The straight lines show the average amount of improvement for any given initial accuracy.

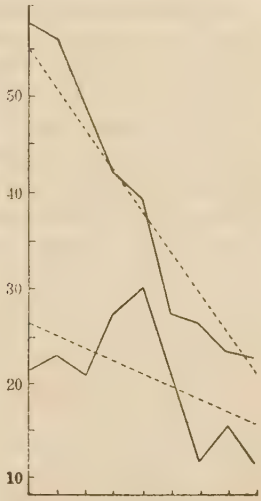


FIG. 12.—Initial and final accuracy of 9 subjects with the rifle, arranged as figure 11, except that the ordinates represent centimeters.

A COMPARISON OF UNLIKE GROUPS OF INDIVIDUALS UNDER THE SAME CONDITIONS OF PRACTICE.

The first experiment was conducted with 8 subjects, each shooting 12 arrows daily under the same conditions. Four of the subjects (Group A) were investigators working in the laboratory, 4 (Group B) were skilled laborers. The men of the first group were all trained in habits of delicate manipulation, such as those required by microscopic technique, and to a much less extent in acts of skill demanding coördination of widely dispersed groups of large muscles. The second group included a good marine engineer, another of less thorough training, a carpenter (acting as cook), and a first-class pilot who had served apprenticeship as a common sailor.

TABLE 34.

	Group A.	Group B.
	inches.	*inches.*
1 to 40 shots	60.1	78.0
341 360 shots	38.6	46.8
Absolute improvement.	21.5	31.2

The distinction between the groups was made primarily upon professional status, and it is not certain that they differed materially in the number and variety of habits of manipulation at their command. Certainly Group A possessed a much greater range and complexity of implicit habits than Group B, while the reverse was probably true of habits of the type involved in archery.

In computing the relative progress of the groups it has been found necessary to omit the record of the writer as involving a different type of learning, and hence not fairly comparable with other records. Acting both as subject and as experimenter, the writer was forced to read such instructions on the use of the bow as were available, and to question the other subjects as to their methods of aiming and loosing. From this it followed that many of the adjustments which the others must make by the method of trial and error were reached by him directly through the mediation of preëxisting language habits. The advantage of this learning with instruction is shown in figure 13,

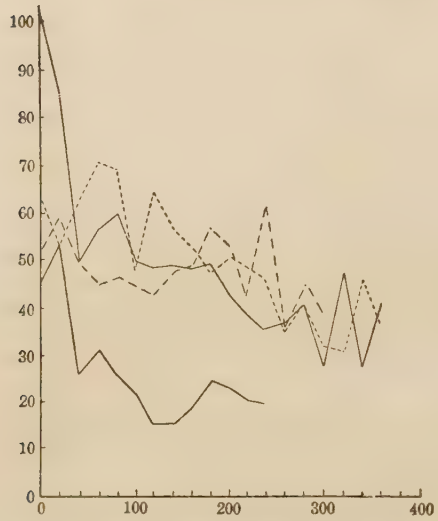


FIG. 13.—Rate of improvement with and without instruction. The heavy line is the learning curve of a subject who received detailed instructions. The lighter lines are typical curves of learning without instruction. Ordinates, distance from the bulls-eye in inches; abscissæ, successive shots arranged by twenties.

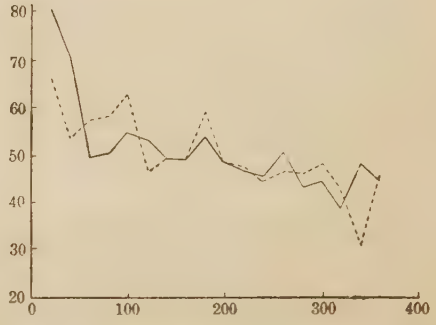


FIG. 14.—Learning curves of group A (.....) and B (——) under light conditions of practice.

where the experimenter's record is compared with 3 other typical curves of learning without instruction. (In this figure, as in all the following learning curves, the ordinates represent the average distance from the bulls-eye of successive groups of 20 shots plotted on the abscissæ.)

TABLE 35.

	Group A.	Group B.
	inches.	*inches.*
1 to 180 shots	55.78	56.91
181 360 shots	44.80	45.77
Absolute improvement.	10.98	11.14

The scores of the two groups, this one record being omitted, are given in table 36 and graphically in figure 14. If the averages of the first and last few shots are taken as indices of the total improvement, there is a fairly large difference in the amount of improvement shown by the two groups. The average, in inches from the bulls-eye, of the first and last 40 shots are given in table 34.

These averages make it appear that the laborers made the greatest absolute improvement, while the investigators showed the greater initial and final skill. From the appearance of the learning curves, it seems probable that the averages of the first and second halves of practice give the truer view of the skill and improvement of the two groups. These averages are given in table 35.

TABLE 36.—*Average distance from bulls-eye of successive groups of 20 shots, for two unlike groups of men under like conditions of practice with archery.*

Shots.	4 laborers.	3 investigators.	Shots.	4 laborers.	3 investigators.	Shots.	4 laborers.	3 investigators.
1 to 20	80.2	66.1	121 to 140	49.3	50.0	241 to 160	50.9	46.8
21 40	70.4	54.2	141 160	49.2	47.5	261 280	43.4	46.5
41 60	49.4	57.7	161 180	54.3	59.1	281 300	44.7	48.7
61 80	50.6	58.2	181 200	48.9	48.8	301 320	38.7	43.2
81 100	54.9	62.8	201 220	46.9	46.9	321 340	48.8	31.1
101 120	53.9	46.3	221 240	45.8	44.9	341 360	44.8	46.2

From this it appears that there is no significant difference, either in the absolute accuracy or amount of improvement shown by the two groups of men shooting under the same conditions.

DISTRIBUTION OF PRACTICE AND RATE OF LEARNING.

PURPOSE AND METHOD OF THE EXPERIMENT.

A year after the end of the experiment just described, with the completion of the Baltimore range, the work was taken up again in a test of the effect of the distribution of practice upon the rate of improvement. 26 subjects were distributed in 4 groups, shooting daily 5, 20, 40, and 60 arrows respectively. Owing to irregularity in attendance of some of the subjects, only 19 records complete to more than 300 shots were obtained, and this fact accounts largely for the differences in the number of subjects and composition of the different groups.

The method employed in the experiments carried out in Baltimore was not greatly different from that used in the Tortugas experiment. The subjects were given only as much instruction as seemed necessary to prevent accident. They were forbidden to discuss their methods of shooting or to seek outside instructions, and in his attempts to follow the various steps in the learning process the experimenter avoided, as much as possible, asking questions which might give the subjects a clue to better methods of shooting. In one respect the technique of the later experiment differed from that of the earlier. The subjects were practically all volunteers and so interested in each other's shooting that it was found impossible to prevent them from watching each other practice. This gave a considerable opportunity for imitation, which was not

offered in the first experiment. The problem of imitation is considered on page 118. Rivalry between the various members of the groups was encouraged and the daily averages were posted on a bulletin board in the shooting-shed.

COMPARABILITY OF THE GROUPS STUDIED.

The subjects for the 5-, 20-, and 40-shot groups were boys and men ranging in age from 14 to 36 years. Those of about the same age were assigned to different groups, with the result that the average age of all the groups is approximately the same. Furthermore, from the data presented in figure 15, it seems clear that there is no correlation between the age of the individual and the rate of improvement of the function, and hence the slight difference in the average ages of the groups (a maximum of 3 years) may be safely disregarded.

The records of a woman are included in the averages of the 5-shot group. Analysis of the individual records shows that the averages of the group were not seriously influenced by the inclusion of this series of records.

With respect to previous training, none of the subjects had ever shot with the bow, beyond the sporadic practice with umbrella ribs and unfeathered arrows which comes within every boy's experience. In training in other functions there was a good bit of variation, but none of the subjects was particularly well trained in athletic sports or in difficult habits of manipulation, and, in general, the groups average out fairly well, with the exception of that whose members loosed 60 shots per day.

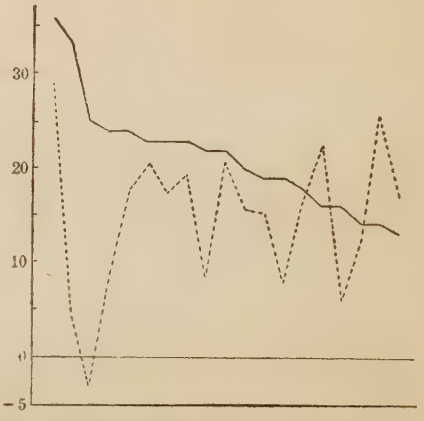


FIG. 15.—Relation of age to rate of improvement. Solid line represents ages of subjects in years arranged in order from left to right. Dotted line gives in inches the absolute amount of improvement for the subject in the corresponding position on the solid line. Averages are computed from first and last 40 shots.

The study of the 60-shot group was undertaken after the others had finished shooting and after their average improvement had been computed. As it seemed that there was some advantage in favor of the 5-shot group, the attempt was made to test the effect of a still greater concentration of practice, continued until quite appreciable fatigue was produced. For this purpose the 60-shot group was organized, but since in the early part of practice this number is excessive, it was necessary to pick subjects who were physically able to meet the exactions of the experiment. As a result, the group was made up of young men ranging in age from 19 to 23 years, in better physical condition than the members of the other groups. The range of variation in age is less in this group than in the others, which fact probably gives an advantage to the group at the start. Furthermore, the superiority in physical strength of the members of this group gives an advantage quite aside from the question of fatigue, in that the bow may be drawn with greater ease and steadiness by an individual whose strength is not greatly taxed by its weight. Sixty shots rep-

resent about the limit that a beginner can shoot without rest during the first few days of practice, and it seemed that if fatigue had played an important part in determining the different rates of learning of the other groups, its effects should be much more marked in the records of the 60-shot group.

The averages of the Tortugas group, shooting 12 shots per day, have been included in the tables for comparison with the later groups, although these averages, like those of the 60-shot group, are not strictly comparable with the others. The average age of the group is considerably higher than that of the remainder, and the range, from 22 to 46, is likewise greater. The practice of the group was carried out under what seemed to be much more unfavorable conditions (at a temperature ranging as high as 110° F. and at times in a blinding glare of light, at others in a very strong wind which carried the arrows several feet from their direct course).

In all of the experiments enough arrows were provided to allow each subject to shoot his full daily number without pause. The arrows were marked individually and arranged in groups of 5, and the results of the shots were

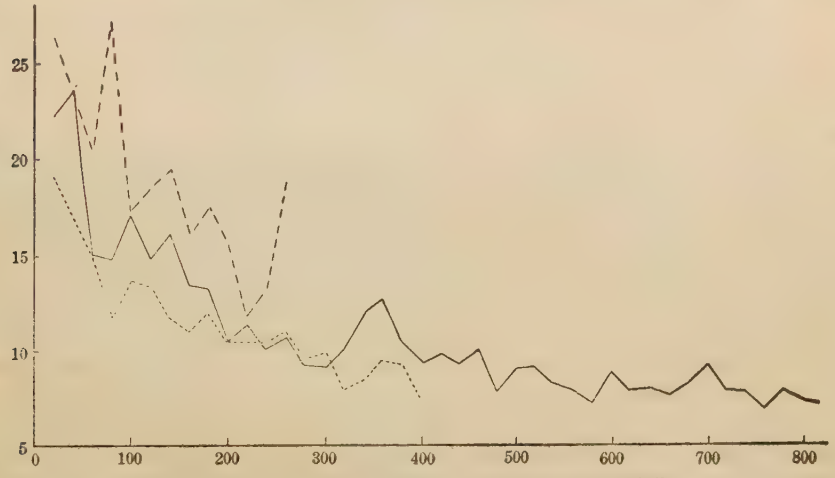


FIG. 16.—Learning with the rifle; all subjects shooting 20 shots daily. ——— 5 shot group; ..... 20 shot group; – – – – – – 40 shot group. Ordinates are distance from the bulls-eye in centimeters; abscissæ, successive shots averaged by twenties.

recorded by successive fives, as it was found to require too much time to keep all of the records in regular order. In the case of the 5-shot group the arrows were recorded in the order in which they were loosed.

Finally, as a control upon the respective abilities of the groups to form habits of manipulation under like conditions of practice, each subject was required to shoot 20 shots daily with the rifle. The daily averages were computed and from these the average improvement of each group was obtained. Owing to the difficulty in securing subjects for the experiment, three individuals who had had some rifle practice were included. One of these was assigned to each group. Their records for rifle practice were not included in the group averages.

The daily rifle scores of the three groups are shown in figure 16. As in the case of practice in archery, it has seemed best to use the averages of the first

and second halves of practice in judging the improvement rather than the records of the first and last few shots (see p. 110). The amount of improvement measured in this way is shown in table 37.

In so far as the improvement in rifle shooting may be considered an index of the relative ability of the groups, the 5- and 40-shot groups seem to be almost equal, the 20-shot group somewhat inferior in rate of improvement. When the percentage of improvement over initial accuracy is considered, as seems justifiable by the data given on page 111, the inferiority of the 20-shot group is less marked.

TABLE 37.—*Relative improvement of subjects shooting 20 shots daily with the rifle.*

[The figures represent centimeters from the bulls-eye.]

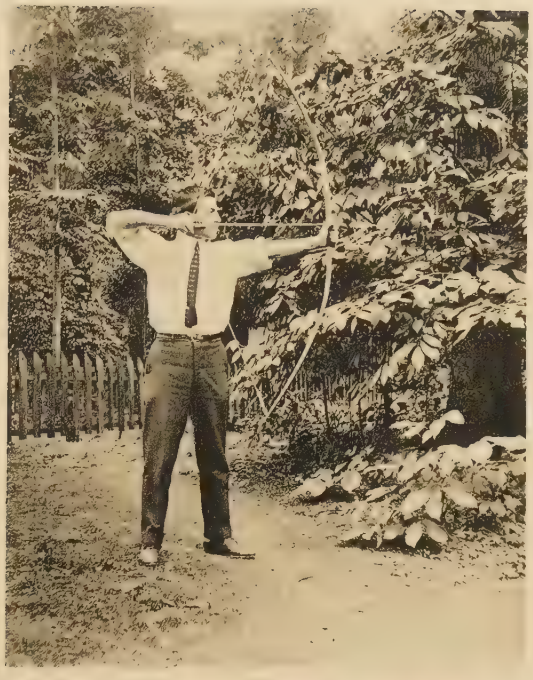
Average of all shots.	Group shooting with the bow. (Shots daily.)		
	5	20	40
From 1 to 140	17.5	14.4	21.9
141 180	11.3	10.8	15.1
Absolute improvement.	6.2	3.6	6.8
Percentage	35	28	31

CONDITIONS AFFECTING IMPROVEMENT.

Before comparing the relative abilities of the groups a few words are necessary concerning the complexity of the habits formed and the general character of the learning curve. Very delicate adjustments, both of the eye-hand coördinations at the moment of aiming and particularly in the quick changes in muscular tension at the moment of loosing, are necessary. The movements involved in loosing the arrow are the most difficult to control exactly. At the moment of aim the extensor muscles of the bow arm are resisting a pull of about 40 pounds; the flexors are under no strain. When the bowstring is released the extensors are suddenly freed from strain and tend to throw the arm outward. The tonus of the flexors must be increased immediately to counteract this tendency. The bow-arm of a beginner frequently swings 4 inches out of position before the arrow leaves the string and the delayed tensing of the flexors then draws it a still greater distance in the opposite direction. This movement must be almost wholly overcome before accurate shooting is possible. Equally accurate and difficult movements of the loosing hand must be acquired. An average variation of 2° of arc in the relative position of the hands results in an average of 25 inches from the center of the bulls-eye. Some of the championship records given below require an average variation of less than 30′ of arc in the alignment of the arrows.

.The record of a subject whose practice extended to more than 1,300 shots is given in figure 17. This is the subject mentioned on page 122 as having had opportunity for practice of implicit habits before the beginning of the experiment. This accounts for the high initial skill and for the resulting flatness of the first half of the learning curve. With the exception of the high initial accuracy the curve seems to be typical for this amount of practice.

1



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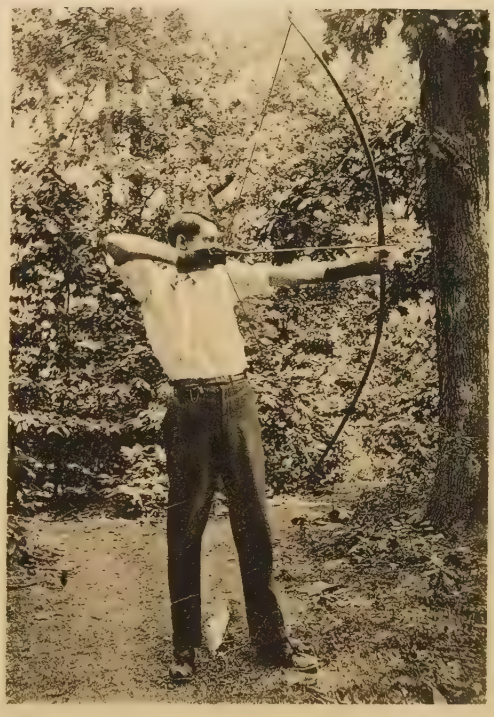

DIFFERENT METHODS OF AIMING.

Fig. 1. Sighting and kinæsthesia combined in aiming. Fig. 2. Aiming without sighting along the arrow.
Fig. 3. Position of the right hand determined by contact with the face.

Improvement after the average of 25 inches is very slow, with the suggestion of a plateau just before the quick improvement after 900 shots. Such periods of no improvement occur frequently in the practice curves. In some cases it has been possible to determine the cause of these with certainty. Thus one subject, after witnessing a slight accident occurring to another in loosing the bow, assumed, to avoid a repetition of the accident, an awkward aiming position and a slow release which interfered seriously with accurate shooting. The aiming position was quickly corrected, but the slow release persisted for many days and kept progress almost at a standstill. In general it seems that the plateaus are the result either of the accidental formation of conflicting habits, or that they represent points where no improvement can be made until a new method of shooting is hit upon by the method of trial and error.

The amount of practice recorded in the experiments was not sufficient to bring the skill of the subjects near the limit of improvement. The final accuracy of about 15 inches indicated in figure 17 is greater than that obtained by any of the other subjects. In comparison with championship records this average is still very high. The championship scores at the distance of 40 yards made in the double American round from 1880 to 1908 vary from 312

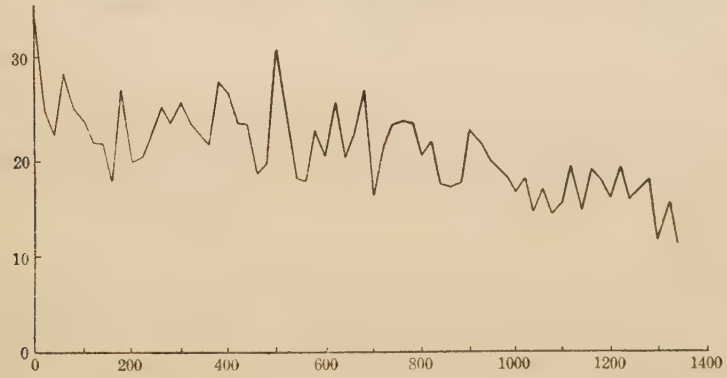

FIG. 17.—Improvement in long practice.

to 412. Reduced to inches from the center of the bulls-eye, they are about 11 and 7.8 respectively. The most consistently good record available is a practice score averaging about 5 inches for 90 shots. Studies of the rate of improvement from 15 to 10 inches are not available, but progress is certainly extremely slow. The writer, after reaching the average of 15 inches, has made scarcely any progress in 1,000 shots. The records used for comparison extend over only the first 360 shots, and deal therefore only with the preliminary stages of learning. During the first 100 shots, particularly, progress is made by leaps. The relation of the bow, hands, and arrow call out suddenly group habits of reaction to geometrical relations; the subject, for example, begins to nock the arrow at a constant place on the string and shows at once a marked improvement in accuracy. Most of the coördinations of larger groups of habits are formed very early in practice and probably account for the negative acceleration in the learning curves.

An attempt has been made to test the effect of fatigue upon the daily improvement. All first shots for daily practice were averaged for the 5-shot

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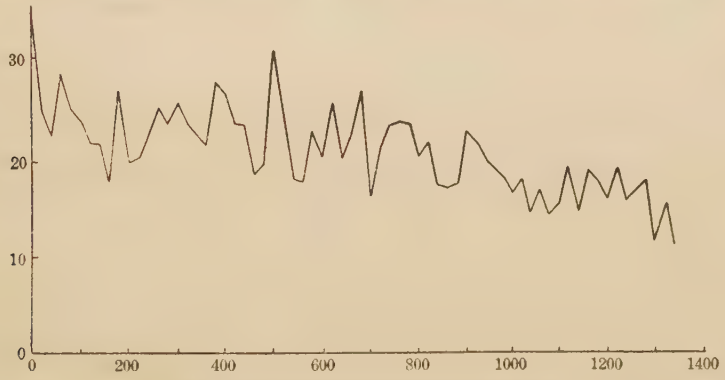

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group. The second, third, etc., shots were averaged in the same way. The differences between the average of the first and fifth shots gives a rough measure of the improvement during practice. In the same way averages were obtained for the 20- and 40-shot groups, but the averages of successive 5 shots instead of single shots were taken, making the average improvement shown a little too small in comparison with that of the 5-shot group. These were then reduced to terms of average improvement during 5 shots loosed in rapid succession. The averages are 1.5 inches for the 5-shot group; 1.5 inches for the 20-shot group; and a negative improvement of 0.2 inch for the 40-shot group. The full data are given in table 38. Little more can be deduced from the results than that fatigue is much more severe after 40 shots than after 20, in the former case obscuring any progress made or perhaps preventing progress (see p. 126).

The question of improvement during periods of no practice is closely related to that of the effect of fatigue in obscuring the improvement during practice. The data have not been analyzed carefully for the improvement during the 24-hour periods of rest, but the effects of some 48-hour periods of rest have been examined. In the 5-shot group there was a decided loss during these periods in the case of 3 of the 4 subjects. The fourth showed a slight gain. The absolute amount of gain was

+1.5 −5.0 −3.5 −3.2

With the results not obscured by fatigue there is a decided loss after practice is stopped.

For the 40-shot group the average gains during 48-hour periods without practice were

−0.5 −0.7 +2.5 +5.5

The record of the individual making the greatest progress during this period is given in figure 18. The solid lines of the figure connect the averages of the first and second 20 shots loosed daily. The dotted lines represent the intervals of no practice. The effects of fatigue are very pronounced in this case. Other individuals of this group show such effects to a less marked degree.

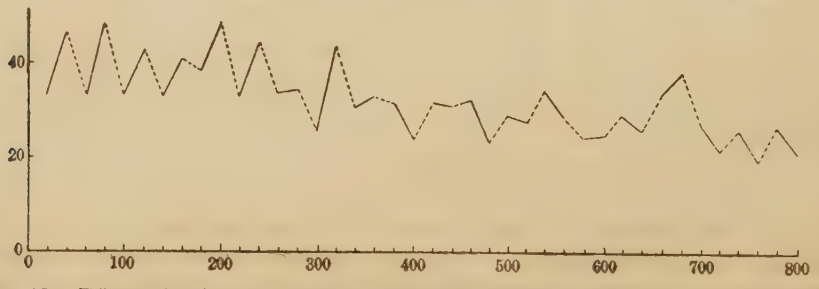

FIG. 18.—Effects of fatigue. Learning curve of one individual in 40-shot group is shown, plotted for successive groups of 20 shots. Solid lines show improvement during practice; dotted lines, improvement from one day to next.

The question of the part played by imitation in the improvement of the subjects is an interesting one. They watched each other's practice and the practice of the experimenter during the greater part of the experiment. From the different methods of aiming employed by different subjects it seems that

imitation played no large part in the learning. Plate 9 shows the different methods of aiming which gave equally good results within the limit of practice. In the one case (figure 1) the alignment was obtained by sighting along the arrow, while the elevation was determined from the position of the right hand in terms of kinæsthesia; in other cases the subjects looked only at the bulls-eye and determined the relative positions of the hands almost wholly by kinæsthetic stimuli (figure 2). In the method of aiming shown in figure 3 the position of the right hand was determined by contact with the face, almost the whole adjustment being made in terms of visual and tactual stimuli. In the

TABLE 38.—*Effect of fatigue.*

		Average of successive shots in daily practice.					Average.
							inches.
5-shot group	1	32.7	38.7	24.2	26.4	....	30.5
	2	20.2	37.5	26.9	27.4	....	28.0
	3	28.9	35.4	28.8	25.3	....	29.6
	4	25.1	36.8	22.4	28.2	....	28.1
	5	22.7	43.2	24.9	25.1	....	29.0
Average improvement in 5 shots.....							1.5
		Average of successive groups of 5 shots in daily practice.					Average.
							inches.
20-shot group	5	26.9	44.4	50.2	50.2	48.5	55.5
	10	25.5	43.1	46.6	44.3	43.5	50.7
	15	23.3	41.4	46.3	47.5	45.1	50.9
	20	25.2	45.3	46.1	41.2	39.7	49.4
Average improvement in 5 shots.....							1.5+
		Average of successive groups of 5 shots in daily practice					Average.
							inches.
40-shot group	5	36.4	37.1	53.1	42.1	....	42.8
	10	30.8	31.0	54.1	50.2	....	41.5
	15	35.4	31.2	53.2	42.7	....	40.6
	20	29.0	31.1	48.5	46.4	....	38.7
	25	32.1	45.7	41.1	40.2	....	39.8
	30	31.3	41.6	54.6	45.1	....	43.2
	35	27.2	38.0	45.0	43.0	....	38.2
	40	31.4	46.1	50.8	49.5	....	44.4
Average improvement in 5 shots.....							−.2

case of every subject some such peculiarities of aiming and loosing could be distinguished, and in most cases the aiming position could be traced out as the result of adjustment by trial and error to difficulties occurring in the early part of practice. Thus the position in figure 1 was assumed originally to prevent the arrow from falling off of the left hand when the bow was drawn.

The great opportunity offered in the experiment for imitation and the small extent to which it seems to have been used raises the question of the real value of imitation in human learning. It is hoped that the further use of archery will furnish more adequate evidence upon this point.

EFFECT OF THE DISTRIBUTION OF PRACTICE.

The group shooting 5 shots daily was organized first and their practice continued for about 2 weeks before the 20- and 40-shot groups started. Their practice extended, altogether, from June 22 until September 20. Practice could not be carried out on Sunday and comparable records of 360 shots were obtained from the 12 weeks' practice.

The 20- and 40-shot groups were organized at the same time and their practice was continued for 4 weeks, giving records of approximately 400 shots for each subject in the 20-shot group and 800 for each in the other group. Comparisons have been made only of the rate of improvement, measured in various ways, in the amount of practice represented by 360 shots. A more certain method of comparing the groups would have been perhaps to continue the practice of each group until a given degree of accuracy was attained and then comparing the amount of practice necessary to reach this stage, as was done by Ulrich in his study of the rat, but this method is practicable only when the final accuracy selected is near the limit of improvement, and the time requirements for such an experiment could not be met.

For purposes of comparison the records of the groups have been averaged in series of successive 20 shots and the curves in figure 19 have been plotted upon the basis of these averages, which are given in table 39. The daily averages are disregarded in this treatment of the data, which consist of averages for like amounts of practice. The records of the 12- and 60-shot groups are included for comparison with these others, although, as has been shown, these groups are not strictly comparable with the remainder.

TABLE 39.—*Averages of successive groups of 20 shots for the groups of subjects compared.*
[Figures are in inches from the center of the bulls-eye.]

Arrows loosed per day.					Arrows loosed per day.				
5	20	40	60	12	5	20	40	60	12
56.9	58.4	55.8	48.9	75.4	35.6	40.9	40.3	33.1	48.9
40.1	49.2	47.8	44.4	66.1	30.9	45.4	33.5	29.6	46.9
39.2	45.3	44.7	39.7	51.5	27.1	32.9	42.6	32.2	45.5
35.2	43.6	46.6	36.2	54.0	30.7	36.1	40.0	30.6	49.6
43.1	40.9	41.8	35.7	60.1	25.6	36.7	38.3	29.1	44.5
39.6	38.8	42.4	36.6	50.7	28.9	38.3	38.9	31.3	46.0
40.7	43.6	36.9	33.9	50.7	35.7	35.3	40.6	32.6	40.2
40.8	40.8	39.1	32.8	48.7	31.4	38.6	37.8	29.1	44.3
35.4	36.2	38.4	32.1	55.9	27.1	44.5	34.9	32.7	45.1

From inspection of the curves in figure 19 it appears that the 5-shot group is considerably superior to the others in final accuracy and in the amount of improvement, possibly excepting the 12-shot group. The superiority of this group is really somewhat greater than is apparent from the curve, for the sudden loss in accuracy coming at the 320th shot is the result of a few days of extremely cold weather, which stiffened the fingers of the subjects and made accurate shooting impossible.

The 60-shot group shows a greater average accuracy but less improvement than the 5-shot group. The 12-shot group shows a much lower accuracy than

any of the other groups, with a somewhat greater amount of improvement. The improvement shown by the 12-shot group is in part spurious, owing to the method of recording the shots in this group. At the beginning of practice many arrows fell short. These were recorded as *failures*. In computing the averages these failures are counted all as 84 inches from the bulls-eye, which was, probably, an overestimate, and added something near 6 or 8 inches to the average records of the first 60 shots.

The amount of improvement in the 5 groups has been expressed in three ways: by the difference in average between the first and last 5 shots fired; between the first and last 40 shots; and by the differences in the averages of the first and last half of practice. These results are given in table 40.

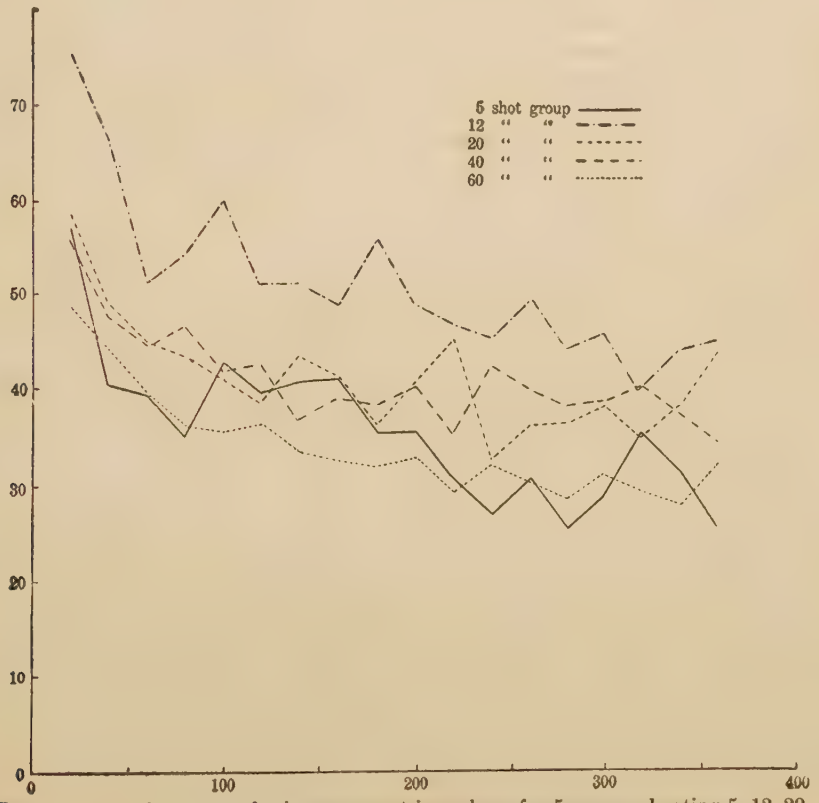

FIG. 19.—Learning curves for improvement in archery for 5 groups shooting 5, 12, 20, 40, and 60 shots daily. Arranged as in figure 13.

The absolute improvement, measured by any of these methods, is greatest in the 12- and 5-shot groups. The 60-shot group comes next with considerably less improvement, and the 20- and 40-shot groups are about equal. It appears from this that the rate of improvement per unit of practice is somewhat greater when the practice is distributed over many days than when it is concentrated into a few days. Whether there is a closer correspondence between the distribution of practice and the amount of improvement is not brought out by this method of treating the results, but an analysis of the data in a some-

what different way indicates that there is probably a fairly close correlation between distribution of practice and rate of improvement when chance causes of variation in the records can be eliminated.

During the first part of practice improvement is made largely by a series of what Thorndike has called "insights," such as those found so constantly by Ruger in the solution of mechanical puzzles. The sight and feel of the bow, arrow, and target present certain elements in common with the stimuli which call out many habits of manipulation and habits dealing with space relations and so tend to call out the same movements. Thus habits of aiming with the rifle are transferred to sighting along the arrow and need little modification to become efficient in producing accurate shooting with the bow. Such coördinations of complex preëxisting habits occur early in the practice of every subject, but whether they come before a single shot is fired, at the fifth shot, or at the sixtieth shot is largely a matter of chance. The subject whose record is shown in figure 17 had opportunity to see the practice of the 5-shot group for two weeks before his own practice was begun, with the result that many of the simpler problems of aiming were worked out in terms of the speech mechanism before he loosed a single shot. The result was somewhat the same as learning with instruction, as shown in figure 13.

TABLE 40.—*Improvement in accuracy after 360 shots with the bow.*

[Averages are in inches from the center of the bulls-eye.]

	Average of shots.				
	Group 5.	Group 12.	Group 20.	Group 40.	Group 60.
From 1 to 5.....	69.2	94.2	69.8	66.0	61.4
356 360.....	34.0	46.6	49.2	39.2	32.3
Improvement.	34.2	47.6	19.6	26.8	29.1
From 1 to 40.....	48.5	70.7	53.8	51.8	46.6
321 360.....	29.2	44.7	41.5	36.3	30.9
Improvement.	19.3	26.0	12.3	15.5	15.7
From 1 to 180.....	41.2	57.1	44.1	43.7	37.8
181 360.....	30.3	45.7	38.7	38.5	31.2
Improvement.	10.9	11.4	5.4	5.2	6.6

These factors result in a greater variability in the earlier part of practice than in later practice, which is independent of the distribution of practice, for few large groups of habits are added after the first 100 shots, as is evidenced by the fact that in later practice the subjects can rarely tell to what changes in their method of shooting their improvement is due, while in the early stages of practice such exclamations as "Oh, I've caught on to something," are frequently followed by sudden large increases in accuracy.

The elimination of the first trials, then, probably gives a truer picture of the effect of the distribution of practice than the inclusion of the earlier practice period. The averages of the first, second, and last third of practice have been computed for each group and are given in table 41.

As in all other methods of treating the data, practice with few shots per day seems to be more economical than that with many. In the first half of practice there is no close correspondence between the amount of improvement and the distribution of practice, but in the second half, where the effects of variations in the initial stage of learning are omitted, the correspondence is quite striking, the 60-shot group alone being out of order. The position of the 12-shot group, as inferior to the 5-shot group, is very probably correct, since in this case the error due to the overestimate of *failures* (p. 121) is no longer significant. The order of improvement per unit of practice is, then, 5, 12, 20, 60, and 40, with unrelated evidence to show that the 60-shot group was somewhat superior to the others.

To what extent do these results express the effect of the distribution of practice, and to what extent may they be due to chance? As has been stated, the 5-, 20-, and 40-shot groups seem very closely comparable in their make-up. The rifle practice, though not very dependable as an index to the abilities of the individuals, does give some indication that the relative rate of learning of the groups practicing under similar conditions is not the same as that

TABLE 41.—*Improvement duing the first and second halves of practice.*

Average of shots.	Shots per day.				
	Group 5.	Group 12.	Group 20.	Group 40.	Group 60.
	inches.	*inches.*	*inches.*	*inches.*	*inches.*
From 1 to 120.....	42.35	59.60	46.03	46.53	40.25
121 240.....	35.08	49.40	39.96	38.47	32.29
241 360.....	29.90	44.95	38.25	38.42	30.90
Improvement:					
First half.....	7.27	10.20	6.07	8.06	7.96
Second half.....	5.18	4.45	1.71	.05	1.39

obtained in the archery practice, which differed from the practice with the rifle only in its distribution, and thus that the differences in the rate of learning to shoot the bow are due primarily to differences in the distribution of practice. If the rifle records are to be trusted as an index to ability, it appears that the improvement of the 20-shot group should be counted as relatively somewhat greater than that of the 40-shot group, since the latter seems to show superiority in rate of learning under like conditions of practice.

From the results of this experiment it is certain that practice distributed over many days is more economical than when concentrated to a few days. From the improvement shown in the later part of practice and general considerations of the relative abilities of the groups, it seems probable that there is a very close relation between the distribution of practice and the amount of improvement.

THE PROBLEM OF "DISTRIBUTION-RATE" RELATION.

These results agree very well with the general findings for the effect of the distribution of practice in the formation of other types of habits. Thorndike has summarized the more important work done before 1910, and it is unnecessary to review this work here in detail. In general, studies upon human learning indicate that the amount of improvement for a given amount of practice is directly proportional to the time interval between the practice periods and inversely proportional to the length of the practice periods, but there is some conflict in the results of experiments upon different types of habits. The results of a few experiments fail to show these relations, but do not seem to be conclusive.

For students learning to write English words in German script Leuba and Hyde give the results shown in table 42 for four groups practicing for periods of equal length but at different intervals. The group practicing every third day alone fails to show the advantage of distributed practice, and not enough subjects were used to eliminate the possibility that this is only a chance variation.

TABLE 42.

Group practicing.	Gained from fifth to tenth practice period.
	letters.
Twice a day.....	240
Once a day.....	290
Once in 2 days....	395
Once in 3 days....	235

Lyon finds that short practice periods give better results in the formation of language habits, except in the case of short verse and prose selections whose meaning can be grasped as a unit; these are learned most readily by concentrated practice. In the case of such material, involving preëxisting language habits of almost inconceivable complexity, it seems very questionable whether the learning process is comparable to the formation of simpler habits.

Hahn and Thorndike found no advantage in favor of either of two distributions of practice, but their groups of subjects are not strictly comparable. The habits studied were those involved in computation and in every case the group with concentrated practice had the greater number of preëxisting habits, which may have obscured any disadvantages due to the concentration of practice.

With the exception of these cases the evidence favors the belief that the rate of learning varies inversely as the concentration of practice. The majority of investigators seem to believe that this holds true only within certain time limits, but with the possible exception of the results of Leuba and Hyde, no such limits have been established experimentally. The types of habits subject to the "distribution-rate" relation and the time-limits of the relation are not yet settled and the solution of the problem will demand a closer inquiry into the physiological causes of the relation than has yet been made.

The following quotation from Starch summarizes practically all the explanations which have been advanced to account for the effect of the distribution of practice upon the rate of learning:

> Why are shorter and more numerous periods economical? The main reason, no doubt, is the well-known fact that a period of rest after newly formed associations gives them a chance to become settled and fixed. The slower rate of improvement of the third and fourth groups is due in part to fatigue. The forty-minute group shows no gain in the last

period, and the two-hour group shows no improvement after the first hour. A third minor factor was that those working for a short period at a time were more apt to work with maximum concentration than those working for longer periods.

The majority of investigators have been content to ascribe the results obtained to "fatigue" or "interest," without attempting further analysis. In planning further experimental work the writer has found the following classified list of possible explanations helpful. It probably is not exhaustive, but may help to define the problem more accurately.

TRIAL AND ERROR.

A. Variety of Proprioceptive Stimuli.

When an organism is confronted with a new set of conditions its reaction is the summation effect of the elements of the external stimuli and the momentary proprioceptive stimuli. The latter may remain fairly constant during a single practice period, resulting in a rather stereotyped reaction. During the relatively long interval between practice periods the proprioceptive "set" may change and thus practice distributed over several days may offer the possibility of a greater variety of activities (some of which may lead to improvement) than the same practice confined to a period during which the same "set" persists.

B. Loss of Conflicting Habits.

W. F. Book has suggested that improvement during intervals without practice is due to the dropping out of habits which make for low efficiency and which have not had time to become well established, while the principal successful actions, more firmly established by longer practice, persist. Such evanescent habits, by restricting the variety of activities, may delay progress considerably and give a decided advantage to the interrupted practice.*

C. Change in the Primary Stimulus.

For successful learning by trial and error it is necessary that the organism perform diverse activities in response to a given set of stimuli. In experiments with animals such as those of Ulrich, the primary stimulus to this activity is furnished by hunger, pain stimuli, unfamiliar surroundings, etc.; in man by the many sublimations of instinctive reactions which constitute "interest," "fear of ridicule," "rivalry," etc. The number and variety of trial movements may depend upon the force of the primary stimulus, either as a result of the amount of diverse activity produced or of the concentration of the activity in responses to a limited number of stimuli, as when the hungry rat spends its time before the door of the problem box and is not distracted by the movements of other rats; or when the human subject keeps his attention strictly on the problem in hand. "Loss of interest through fatigue" probably represents a change in the intensity of the primary stimulus to activity.

*The appearance and disappearance of habits of this type have been noted frequently during the experiment. For example, a subject assumes an incorrect aiming position in order to avoid catching the bowstring on his sleeve. This position persists for some time after the sleeve is rolled back out of the way, and may even become firmly fixed.

DIRECT EFFECTS OF FATIGUE.

Fatigue may have an effect upon the rate of learning in other ways than by changing the intensity of the primary stimulus.

A. Neurone Patterns.

It is probable that in order to obtain a given result different groups of neurones must be employed when the organism is fresh from those employed when it is fatigued. Thus, subjects in the 40- and 60-shot groups were seen to adopt different attitudes in aiming as they grew tired. The result of this is that a greater number of coördinations must be made to produce the same degree of skill whenever practice involves much fatigue.

B. Muscle Changes.

In archery there is a possibility that the effect of the distribution of practice is a function of muscle growth under different conditions of exercise. There seem to be no adequate studies on this subject.

PRACTICE BETWEEN PRACTICE PERIODS.

Where the subjects are interested in the experiment it is impossible to prevent their thinking and talking about it during the intervals between practice periods. This is the suggestion made by Thorndike in respect to Munn's results for language habits in substitution tests. It seems to apply equally well for the early stages of habits of manipulation in man (see p. 122), but its application to the rat in the maze test is questionable.

FIXATION OF THE NEURAL ARC.

Starch's first suggestion quoted above implies that a single activation of a neural arc starts up a process of fixation which continues for some time and that the further functioning of this arc during the process of fixation does not accelerate the process of fixation proportionately. By way of illustration, the following analogy may serve: Each time that a door with rusty spring hinges is opened it swings more easily. But when one man has opened the door others may follow him before it swings shut without wearing the hinges smooth. It is only when some time intervenes between the passage of the men that each reduces the friction in the hinge equally.

THE TIME RELATION OF PRACTICE TO THE CHANGES IN THE PRIMARY STIMULUS RESULTING FROM SUCCESSFUL ACTIVITIES.

The conception that the fixation of a habit is the consequence of its pleasurable result has been stated in a somewhat objective form by Ladd and Woodworth in a discussion of the escape of the animal from the problem box, as follows:

> We must assume in the animal an *adjustment* or determination of the psycho-physical mechanism toward a certain end. The animal desires, as we like to say, to get out and to reach the food. Whatever be his consciousness, his behavior shows that he is, as an organism, set in that direction. The adjustment persists till the motor reaction is consummated; it is the driving force in the unremitting efforts of the animal to attain the desired end. His reactions are, therefore, the joint result of the adjustment and of stimuli from various features of the cage. Each single reaction tends to become *associated* with the adjustment. But the unsuccessful reactions are less strongly associated than the successful, because each one of the former is at some moment given up or inhibited; and this inhibition, too, being

made under the influence of the adjustment, tends to become associated with it, and so to interfere with the association between the adjustment and the performance of this particular reaction. In the case of the successful reaction, however, the phase of inhibition does not occur and the only association with the adjustment is of the positive sort.

If habit fixation takes place in this way, progress perceptible to the subject should cause a quicker fixation of the habit. It is generally stated that long-practice groups can make an appreciable improvement in one practice period; that on this account their practice is more satisfying; and that this should result, therefore, in quicker learning. This would tend to produce a "distribution-rate" relation the reverse of that found.

In the present experiment the evidence makes it seem highly improbable that the "distribution-rate" relation was the result of differences in the primary stimulus. Members of the 5-shot group showed no greater interest in the problem than did the others; indeed, toward the end of practice, it seemed to the experimenter that the members of this group, after two months of faithful practice, were beginning to find the work something of a bore. The members of the 12-shot group were very busy with other work and seemed less interested in practice than any of the remaining groups. Yet these two groups made the greatest progress.

So far as the other possibilities are concerned, the present experiment offers little that is helpful in eliminating any of them. The effects of fatigue probably should not be emphasized too much. The 40- and 60-shot groups suffered far more from fatigue than did any of the others and their slower rate of progress does not seem proportionate to the amount of fatigue resulting from such long-continued practice. Changes in the muscles themselves, while possible in archery, are almost certainly precluded from language habits.

Which, if any single one, of the possibilities suggested is the real cause of the relation between the rate of learning and the distribution of practice, the evidence at hand is not sufficient to decide. Certainly some seem more, others less probable, but it is not certain that any of the four chief classes mentioned has been absolutely eliminated from any of the experiments thus far completed.

RÉSUMÉ OF RESULTS.

(1) The rate of learning to shoot with the bow was studied in two groups of men, one of the artisan, the other of the professional class. No significant difference in the rate of learning in the two groups was found.

(2) The rate of improvement of five fairly comparable groups during practice of the same function was tested under different conditions of practice. A close correspondence between the distribution of practice and the amount of improvement appeared, a given amount of practice being more efficient when distributed through many short periods than when concentrated in a few long ones.

(3) An attempt has been made to define the physiological problems involved in this "distribution-rate" relation.

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